

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—MORAL AND NON-MORAL VALUES: A
STUDY IN THE FIRST PRINCIPLES OF
AXIOLOGY.

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1.

It would, I suppose, be pretty generally agreed that the fundamental cleavage within value-philosophy at the present day is between those who hold that goodness (or value) is a simple unanalysable—and hence indefinable—*quality* which certain things possess, and those who hold that it consists in a *relation* of some kind between the things of which value is predicated and some mind or minds. As labels will be convenient for views of which we shall have much to say in the sequel, let us call these two schools of thought the 'Objectivist' and the 'Subjectivist' respectively.

Of the schools, the Objectivist is, of course, by far the more recent—dating virtually from the publication of *Principia Ethica* some thirty years ago. And I do not think it unfair to suggest that its considerable authority has been due much less to its own positive merits than to certain apparently irremediable defects in the Subjectivist theories which seem to offer the only plausible alternatives. Its authority can hardly be accounted for by the logical arguments which appeared to Dr. Moore in 1903 to prove irresistibly that goodness could only be a simple quality: for these arguments have not, as a rule, been found satisfactory even by those who are most sympathetic with the general trend

of the theory, and they are not now, one learns,¹ acceptable to Dr. Moore himself. Nor is the doctrine qualified to attract adherents by any power of affording an intelligent comprehension of the vast and varied panorama of value-judgments disclosed to us by the student of comparative cultures : for this is just what any view of value which denies the relevance of subjective interests is singularly incompetent to do. Indeed, the advantage of Subjectivism with respect to this important requirement of a sound value-theory is at once so great and so evident that only the presence of very grave counter-balancing disabilities seems able to explain the preference accorded in so many quarters to the claims of the Objectivist type of theory.

It must be admitted, however, that very grave difficulties do beset the path of the Subjectivist. Two, I think, are outstanding, and are constantly being adduced by critics as manifestly fatal to the pretensions of Subjectivism. Let me state them very briefly.

(1) If what we mean when we predicate goodness of X is that X is liked (or desired, approved, etc.) by some mind or minds, then goodness must be something of a highly contingent character. It must, apparently, be something that comes into being and passes away not only (as is natural enough) through changes in the nature of X, but also on account merely of changes in certain conscious states. And it is very difficult to believe that this is what we do in fact mean, at least in our more considered and unqualified value-judgments. When we assert that knowledge is good, or that beauty is good, surely we do not mean that knowledge and beauty own this character only if and when certain conscious states are directed towards them? Is it not evident that we regard the goodness of each of these 'goods' as possessed of the same kind of permanence as the nature of the thing itself, and not as something which fluctuates with the fluctuation of any person's, or group of persons', feelings towards the thing?

(2) Among conventionally accepted good things there is one whose goodness we feel it quite peculiarly repugnant to identify with any relation to subjective interests. I refer to moral virtue. In the case of other good things—such as knowledge—we at least do not feel that there is anything inherently absurd in enquiring whether the goodness we attribute to the thing may not be derivable from its being an object of interest to some subject or subjects (or from its being a means to the attainment of some such object of interest). However difficult it may be to devise a formula which avoids the difficulty alluded to under (1), we at least recognise nothing crassly incongruous in undertaking the

¹ *Proceedings of the Aristotelian Society*, Supplementary Vol. XI, p. 127.

attempt. But the case of moral virtue appears to fall into quite a different category. When a man in defiance of strong temptations rises to what he recognises to be his duty, it seems merely inept to suggest that his dutiful act derives the value which we all regard it as possessing from any subjective feelings that are entertained towards it by any one. We seem to see quite clearly that we need pay no attention to the presence or absence of any subjective feelings whatsoever in order to know that the act has value. Whether or not the act may have some *further* value in virtue of a relation to subjective feelings, we may here be content to leave as an open question. But it can only be by confusion that we fail to distinguish the 'supplementary' value which the act may possess on *this* account from the value which it possesses simply and solely in virtue of its being the kind of act that it is, *i.e.*, an act of duty.

Now I do not believe, for reasons which I shall later explain, that the former of these difficulties is really insuperable. But I confess that the latter difficulty does seem to me fatal to any purely Subjectivist theory of value. It is, I think, quite hopeless to seek to identify the value we attribute to moral virtue with any relation to anything that can legitimately be called a subjective interest. Indeed, the simple appeal to reflect upon our own value-responses to moral virtue seems to me to be by far the most effective weapon in the whole armoury of the critics of Subjectivism, and it has probably been responsible for making more converts to the general Objectivist stand-point than all other arguments put together. It is very noticeable—and, as I shall attempt to show, highly significant—how frequently it recurs at critical junctures in the pages of Dr. W. D. Ross's recent important work.¹ I cite here only a single typical passage, from the chapter on 'the Nature of Goodness'. 'We may claim,' writes Dr. Ross, 'that we are directly aware that conscientious action, for example, has a value of its own, not identical with or even dependent upon our or any one else's taking an interest in it. Our reason informs us of this as surely as it informs us of anything, and to distrust reason here is in principle to distrust its power of ever knowing reality.'² It is a claim, I think, whose cogency will strike home to almost every reader. But note how immeasurably it would be weakened in its effect if, instead of the *moral* value of 'conscientiousness', we were to insert some non-moral value—even one of the so-called 'intrinsic' values like 'knowledge'. It is possible that, if we were to substitute 'knowledge', we should still feel an *inclination* to assert that this

¹ *The Right and the Good*.

² *Ibid.*, p. 82.

good 'has a value of its own, not identical with or even dependent upon our or any one else's taking an interest in it'. But I doubt if there is one of us who would go so far as to contend that 'our reason informs us of this as surely as it informs us of anything, and to distrust reason here is in principle to distrust its power of ever knowing reality'. The strength of the case against Subjectivism depends, I think, far more than is generally recognised upon the special instance of the value we apprehend in moral virtue.

Still, a single argument, if it be sound, is enough to establish any position : and I am willing to admit, on the strength of the argument we have been considering, that an adequate theory of value is not possible on purely Subjectivist lines. But before we acquiesce in a thorough-going and general rejection of Subjectivism, there is one alternative which, as it seems to me, we ought to explore with a great deal of care. Is it absolutely necessary, I want to ask, that we should extend to the meaning of value or goodness generally those characteristics which we find ourselves forced to apply to it in the case of moral virtue ? Is it quite certain that we mean the same thing by 'good' when we judge that moral virtue is good as we do when we judge that knowledge is good—or indeed as we do in any other of those instances (the crucial ones, of course, for the determination of the ultimate nature of value) in which we predicate goodness in an apparently 'absolute' sense ? If this is *not* so, then it may remain possible to give a Subjectivist account of value in respect of non-moral values, even while we recognise that such an account is definitely false in respect of moral values : a consummation which would deliver us from the embarrassment, which presses so sorely upon the Objectivist school, of holding a theory of value which leaves in outer darkness by far the larger proportion of the value-judgments of mankind.

I hope that the reader will not dismiss this alternative too hastily as savouring of mere convenient eclecticism. It has, I believe, a much more solid basis. Indeed it is, in my opinion (which I shall endeavour to substantiate in this paper), the one view capable of introducing coherence into the general theory of value. I believe that the chief source of the confusion which envelops current value philosophy lies precisely in the failure to recognise that there is an absolutely vital distinction of kind between our value-reaction to the value of moral virtue and our value-reaction to any other value whatsoever.

To explain my thesis in more formal terms, what I am going to argue is that a subjectivist definition of value is valid, *save only*

in the single case of the value which inheres in moral virtue. How exactly we are to understand the meaning of value in the latter connection, and what is the nature of the identity or analogy between it and value in the former connection which leads us to employ the same term 'value', or 'goodness', in respect of both, are questions which must engage our attention at some later stage. But our chief preoccupation will be with the meaning of value in the former connection; and our chief problem in the effort to vindicate a subjectivist interpretation of 'non-moral' values will be to devise a formula in terms of subjective feelings which will escape, along with other difficulties, the major difficulty alluded to at the outset of this paper.

2.

It is perhaps desirable, before we settle to the task of constructing a subjectivist formula for non-moral values, to say something more of a general character in support of the uniqueness of the value which we attribute to moral virtue. Let us begin by trying to give to the difference in kind which we allege, at least the status of an initial probability: for the reader who is already persuaded that this alleged difference at least *may* have a foundation in fact, is likely to explore with considerably greater sympathy the hypothesis that a subjectivist interpretation is valid for non-moral values.

One piece of evidence in support of the validity of the distinction has already been referred to, and I shall develop it only briefly. I quoted¹ a passage from Dr. Ross which purported to show that the value which we accord to conscientious action cannot be given a subjectivist interpretation, and I invited the reader to observe the effect upon the argument of substituting for conscientious action some non-moral value. If we carefully examine our value-reactions in the two cases, it seems to me that the difference between them is very marked, and highly important. We feel, so Dr. Ross contends, completely certain that conscientious action has value irrespective of any subjective interest in it—a value to which subjective liking and disliking are merely irrelevant. I think that very few persons, if any, would maintain that a value like beauty (or, as I should prefer to express it, æsthetic experience) evokes a corresponding assurance. The habit of mind, engendered by a long tradition, which tempts us to take it as established verity that Truth, Beauty and Goodness are a sort of holy trinity of 'intrinsic' values may, and only

¹ P. 275.

too probably will, predispose us to believe in objectivity in this value-realm also. But we surely cannot pretend that we here enjoy anything even approximating to *certitude*. We should not stake the very validity of our reason upon the objectivity of this value. The difference in our attitudes is very easy to understand if the distinction I am contending for is *bene fundatum*. It is not easy to explain on any other hypothesis.

Further evidence may be derived from considering a problem which has evidently very much exercised the author of *The Right and the Good*—the problem of the commensurability of values. If we suppose that the goodness possessed by moral virtue on the one hand, and by non-moral good on the other hand, is goodness in the same sense of goodness, then presumably it should be possible, since goodness admits of a more and less, to measure the goodness of virtue against the goodness of the other goods in at least a rough and ready, approximate fashion. Yet in actual truth it seems impossible to dissent from Dr. Ross's own considered judgment that the goodness of virtue is really incommensurable with the goodness of any of the other 'intrinsic goods'. Critical examination of our own value-responses strongly suggests that we decline to accept any amount, however large, of a good like knowledge as exceeding in value the very smallest amount of moral virtue. 'When I ask myself', says Dr. Ross, 'whether any increase in knowledge, however great, is worth having at the cost of a wilful failure to do my duty or of a deterioration of character, I can only answer in the negative.'¹ And this is surely a correct answer. For my own part, I should say that the very question has about it an unreal, artificial flavour. It is the kind of question which no one could seriously put to himself save in a mood of half-conscious sophistry, or else—as here—under the influence of a philosophical motive which demands the explicit examination of even the most remotely possible alternatives. I am not myself aware of any philosopher, with the possible exception of Dr. Moore—who in theory assigns to moral virtue a very modest degree of value—likely to return an affirmative answer to the question, and it seems fair to take the negative answer as possessing a pretty high measure of certainty.

Now if the goodness of virtue is really felt to be incommensurable with the goodness of other good things, can we still hold that we are meaning by 'goodness' the same thing in both cases? Dr. Ross tries to evade the difficulty, which his own candour has so clearly exposed, by suggesting that 'virtue belongs to a higher order of value, beginning at a point higher on the scale of value

¹ *The Right and the Good*, p. 152.

than that which (the others) ever reach'.¹ But I feel very doubtful whether this supposed relationship will bear examination. If it is really the *same scale* to which they belong—and this condition seems to be necessary if they are to be defended as 'good' in the same sense—two difficulties emerge. In the first place, since it does not seem possible to set any assignable limit to the amount imaginable of goods like knowledge and pleasure, must we not say that at *some* point the goodness possessed by these goods equals the goodness possessed by the minimal amount of virtue? And, in the second place, ought we not to feel that with increasing amounts of knowledge and pleasure we are continuously *getting nearer* to the degree of goodness which the minimal amount of virtue possesses? I think that if the values do indeed belong, as Dr. Ross says they do, to the *same scale of value*, an affirmative answer to both of these questions is logically entailed. Yet it seems to me in the last degree doubtful whether an affirmative answer can be supported by the actual responses of our value-consciousness, and I imagine that Dr. Ross's own recognition of the peculiar claims of moral value must make him extremely reluctant to accept that answer himself.

Now the difficulties, it should be noticed, which beset the attempt to measure the value of virtue against the value of other goods, are precisely what one would expect if the thesis that I am to defend in this paper is a sound one. If virtue is a value not just of a *higher order* but in a *different category*, if it is, in fact, the one thing which has a value independent of all relation to subjective interests, then little wonder that we are aware of an inherent impropriety in seeking to apply a common yard-stick to it and other goods.

3.

It is rather more than time, however, that we passed on to our main task. Our real problem still awaits us, *viz.*, to justify the contention that all value-judgments other than those referring to moral virtue involve an essential reference to human liking.² Let me say a word or two, first of all, about the method I propose to adopt.

¹ *The Right and the Good*, p. 150. In the context the statement relates only to the non-moral value of 'pleasure', but the sequel makes it clear that Dr. Ross intends his view to apply to other non-moral values also.

² I shall explain shortly why, in my judgment, 'liking' is to be preferred in this connection to 'interest' or 'desire', or any other term that Subjectivist axiologists have proposed.

Naturally, I shall not attempt to deal explicitly with every variety of value-judgment. Our main concern must be with those judgments which are especially appealed to by the Objectivists as furnishing indubitable instances of the apprehension of a value which is not reducible to subjectivist terms. There is, indeed, no list common to all the Objectivists; but the claims of knowledge and æsthetic experience (Truth and Beauty, in popular parlance) have received such wide-spread acknowledgment that it is certain that no Subjectivist theory has any hope of acceptance which cannot make clear the disguised relationship to human liking which obtains in their case. If we can find a formula in terms of human liking which can be regarded as representing fairly what is really meant by goodness or value in the value-judgments directed upon these crucial instances, it will probably be agreed that we have surmounted by far the greatest obstacle to the vindication of our general thesis.

Now the formula which I shall venture to put forward is, as might be expected from the nature of the case, of a somewhat intricate character. The one thing abundantly certain is that no simple formula—such as ‘liked by the person judging’, or ‘liked by the majority of the community’—has the faintest chance of withstanding criticism. But, because of its complexity, our formula, if stated baldly at this juncture, would convey very little meaning indeed. I propose, therefore, to lead up to it gradually by undertaking a systematic examination of the nature and growth of our value-consciousness in so far forth as that value-consciousness is rooted in human liking.

For the sake of clearness, let me outline very briefly in advance the main stages through which we shall pass on the way to our goal.

I shall start from what appears to me to be the most rudimentary expression of ‘subjective value-consciousness’—if I may so entitle the value-consciousness that is rooted in human liking—viz., that which is ingredient in simple private liking and which manifests itself in the concept ‘good-for-self’. I shall then endeavour to show how, by an entirely natural development, incited by the growing recognition of certain distinctions which force themselves upon an intelligent subject of experience, that value-consciousness comes eventually to identify with ‘good for self’ only objects of its liking *which are qualified in a highly specific way*. I next turn to consider the reaction of our value-consciousness to the recognition (in practice, of course, present from the start) that other persons also have likes and dislikes, and that there are also likes and dislikes which may be said to be

inherent in our common human nature as such. This leads us to see that our value-consciousness (still operating on the basis solely of human likes and dislikes) will naturally take a vital interest not only in the concept 'good for self', but also in the concept 'good for man', and that what it means by 'good for man' is an object of liking to human nature qualified in a specific way parallel to that which we found to be involved in the developed concept 'good for self'. We then observe that the formula to which we have been led in considering the true meaning of 'good for man' is one which, if applied to the matter of experience, must issue in a list of goods which bears a remarkable resemblance to that which is currently supposed to represent goods that are intrinsic and objective, goods without any relational qualification. This discovery suggests the possibility that the concept which controls our recognition of the so-called intrinsic goods is really the concept 'good for man'. An explanation will, of course, require to be given of how it is that the relational qualification—good *for man*—if it controls our thoughts, does not appear in our speech. If we really mean 'good for man', why do we say just 'good'? This difficulty, however, proves to be superficial. Further difficulties are also considered and rejected, and the final conclusion come to that our real meaning when we seem to be asserting intrinsic goodness of such goods as knowledge, æsthetic emotion, etc., is that they are specifically qualified objects of liking to human nature.

4.

We start, then, from the experience of simple private liking. That which we like, we 'put a value upon', as the common phrase has it. It is, to be sure, only a value-*for-self*, not a value *in itself*. For the value which we attach to the thing merely in so far forth as it is an object of our liking is certainly not regarded by us as a value 'in the nature of things', nor even as a value for anyone besides the person liking. But it certainly *is* regarded as a *value*. It is possible to dispute the relevance of the term 'value' here, only if we quite arbitrarily, and indeed perversely, decide that the term 'value' is to be used as the equivalent of the term 'objective value'. There seems no excuse whatever for such a usage, which can hardly be so much as intelligibly formulated to oneself without drawing a distinction in thought between objective and subjective value; a distinction which, if admitted, at once makes nonsense of the proposed identification of 'value' with 'objective value'.

I take it, then, to be indisputable that there is a fundamental manifestation of value-consciousness which is the consciousness of 'value-for-self'. But before we go any further, a word must be said about the choice of 'liking' as the basis of that consciousness. That the interruption may be as brief as possible, I shall do little more than tabulate the chief grounds (as I see them) for preferring 'liking' to certain other terms that have from time to time been suggested in a subjectivist interest.

If we compare 'liking' first of all with 'desire', the advantage of the former term is two-fold.

(1) We often ascribe value to an object (or state, activity, etc.) understood to be already in existence, and which we therefore—since desire is always for something conceived as not yet existing—cannot be said to desire. Perhaps it will be said that desire is present in such cases, in that we desire the continuance in existence of the object. But clearly that desire is posterior to the consciousness of the value of the object. It is because we value the object, 'like' its existence, that we desire the continuance of its existence. 'Liking' is to be preferred, then, in that, unlike 'desire', it applies indifferently to the existent and the non-existent, just as valuing does.

(2) Desire, as ordinarily understood, is always an *actual* state of mind, whereas there are certainly value-*dispositions*. It is awkward, if not impossible, to say that a person desires an object if he does not have a present conscious attitude towards it. Yet he may certainly be said to value an object under these conditions. We may legitimately say that X values exercise, even if we know that while we say it X is in bed asleep. In this respect too, then, 'liking' shows its superiority to 'desire'. For 'likes and dislikes' signify conative dispositions quite as much as they do present mental states.

The claims of 'pleasure' to be the basis of value-consciousness have some *prima facie* strength. But a fatal objection is that we can certainly feel pleasure without being conscious of an object which we are pleased with, or at, whereas value-consciousness is essentially transitive, implying an object which is evaluated. Our chosen term, liking, obviously possesses the same transitive implication. It might be suggested, perhaps, that 'being pleased with' is the basic state. But 'being pleased with' seems to be just a longer way of saying 'liking'.

Mr. Perry's term 'interest'—to take note of one further possibility—does not lie open to any of the above objections. It is transitive, it can refer to dispositions, and it is applicable both to

the existent and the non-existent. On the other hand, the use to which this term has been put in the science of Psychology has left it with associations misleading in the present connection, and it has not the natural power, which the term 'liking' possesses, to suggest that favouring attitude of the subject mind which is absolutely fundamental. Moreover, it is a disadvantage that the term 'interest' is applied indifferently to the psychical state and to the object of the psychical state. And, finally, it would be inconvenient to be precluded from using the term 'interest' when we wished to signify that cognitive attentiveness which the term is commonly used to signify.

'Liking', then, seems on the whole to be the best term to express the basic element in the consciousness of value-for-self. We must now take up our task of tracing the manner in which the concept of value-for-self acquires for the reflective consciousness a much more specific meaning than mere 'object of self's liking', owing to the recognition of certain distinctions within objects of liking. I shall set out these distinctions in logical rather than historical order, beginning with the most simple and working towards the more complex.

The first distinction is the very elementary one between objects liked *more* and objects liked *less*. This distinction need not detain us. It introduces, in its crudest form, the distinction between major and minor values-for-self, corresponding to objects of major and minor liking respectively. It ought not, in strictness, to have any effect in modifying the meaning of the concept 'value-for-self'; although I shall have to point out later that it is not altogether certain that it does not in fact have some slight modifying influence.

The next distinction has greater importance. There are some objects that we like for themselves, others that we like only because they help in the attainment of objects liked for themselves. Recognition of this distinction leads us to make a distinction between *end* values-for-self and *instrumental* values-for-self, corresponding to objects of independent and dependent liking respectively. And with the emergence of this distinction the meaning of 'value-for-self' does begin to undergo definite modification. Since it is from end values that instrumental values derive all the value that they possess for us, it will be natural to recognise that it is only end values that have a direct claim to the title 'value'. In so far as this distinction is active in the mind, therefore, there will be a tendency to identify value-for-self not with object of *any* liking of the self, but with object of an *independent* liking of the self.

Our next distinction, one which has very important consequences, arises within the field of end values. It rests upon recognition of the fact that some end values have *also* instrumental value, being conducive to the attainment of certain other things liked for themselves, while other end values have from the same point of view a definite *disvalue*. Thus ends like health and knowledge may be liked for themselves, but liked *further* because seen to contribute usefully to the attainment of many other liked things, whereas a good many things liked for themselves, *e.g.*, idleness and gluttony, have quite obviously an opposite tendency. We may perhaps express the situation that arises by saying that end values fall roughly into two classes, according as their main tendency is to co-operate with or to obstruct the end values of the self as a whole.

Now the 'co-operative' end values, if we may so christen them, will certainly be accorded a much higher status as values-for-self than the 'obstructive' end values. But the precise nature of this 'higher status' is something which we must determine with a good deal of care, for in truth we meet here with a very important development in the meaning of the concept 'value-for-self'. There is no question, indeed, of any modification in principle of the original equation 'value-for-self' = 'liked by self'. But a complexity now reveals itself within the meaning of 'liked by *self*' which reacts profoundly upon the meaning of 'value-for-self'. For it now appears that 'liked by self' may mean either, on the one hand, to be an object of liking to the self as a whole, to the self as the unitary centre of its several likings—as in varying degrees is the case with what we have called the 'co-operative' end values—or it may mean, on the other hand, to be an object of liking to the self only in some very partial aspect of its being, and so inimical to the self's other likings as to be more properly called an object of *disliking* to the self as a whole—as in the case of the 'obstructive' end values. Now it can hardly be denied, I think, that it is in the former of these two conceptions of itself, *i.e.*, in its being as the unitary centre of manifold likings, that the self recognises its essential selfhood to consist. Accordingly, it will be only those objects of liking which are harmonious, if not positively at least negatively, with the self's likings as a whole, objects which belong to the class of co-operative end values, or, at the least, of neutral end values, which will now be accepted by the self as genuinely representing what is 'liked by *self*'; and it is they alone which will now be recognised as 'values-for-self'. It is evident that when this distinction has become operative in the mind many things previously regarded

as values-for-self will present themselves quite definitely in the light of *disvalues*: because, though in one sense still 'liked by self', in a more profound sense of 'self' they are in antagonism to what is 'liked by self'.

The distinction which is now engaging us concerns our ultimate purpose so closely that I may be excused if, in spite of the limited space at my disposal, I dwell for a little upon its general principle. The general principle is, I think, neither obscure nor seriously debatable. The essence of the matter is just this, that as self-consciousness develops, and the self becomes conscious of itself as the unitary centre of manifold likings, the meaning of 'object of liking to self', and consequently of 'value to self', becomes deepened, and in a manner transformed. Whatever is now regarded as good or valuable for the self has got to be something that respects the systematic manifoldness that belongs to the nature of a self. 'Good-for-self' will now mean object not merely of an independent liking, but of an independent and *integral* liking of the self—an 'integral' liking being definable as one which is substantially consistent with the likings of the self as a whole.

We must note now, but more briefly, a further modification of the meaning of value-for-self which arises at the same level of reflective self-consciousness as that which we have just considered. At this same level, the self will become explicitly conscious of its perduring identity, conscious of itself as a relatively *abiding* subject. For a self which so understands its self-hood, an object of liking will tend to be regarded as fully deserving the title of object of the *self's* liking only in so far as the liking in question is of a relatively permanent and not a merely ephemeral character. It would appear, therefore, that the epithet 'relatively permanent' ought to be added to the epithets 'integral' and 'independent' in order to denote accurately the kind of object of liking which is on this developed plane of experience identified with 'value-for-self'.

There is just one other distinction which must, I think, add its quota of meaning to the concept 'value-for-self'. The immediately preceding determinations rested upon the self's consciousness of a distinction between a relatively real and a relatively unreal expression of self-hood. But self-consciousness leads to the recognition of a further distinction within self-hood, the distinction of the self as it is from the self as it is capable of becoming. We become aware of the self containing within itself possibilities of desirable development in a multitude of directions. Now when we consider the significance of this manifestation of our

self-consciousness in its relation to the self's likings, we can see that the self will recognise (1) that there are many things which it does not now like, but which it is in principle possible for it to *come* to like; and (2) that among these things there are some which there is good reason to suppose that it is *worth while* coming to like, since we can even now see their nature to be such that the liking of them is in a high degree integral, and relatively permanent, as well as independent. Thus a person might very well have no present liking for scientific pursuits, or for music, but at the same time, because fully realising their fulfilment of the conditions required for a high degree of value-for-self to any self which does like them, he might *want* to like them. What will be the attitude of the value-consciousness towards such objects of prospective but unawakened liking, objects which it does not now like but would only, as it were, *like* to like? On the whole, it seems probable that their relation to the likings of the ideally developed self will bring them recognition as values-for-self in some sense. Just how much the concept of value-for-self must be modified thereby it is not easy to determine, and it is fortunate that, as will become apparent later, this particular modification has not the importance of its predecessors for the fulfilment of our ultimate purpose.

Now we have so far been studying only that branch of our value-consciousness which is connected with the self's consciousness of its *own* likes and dislikes. But man's life is lived in a social medium, and it is very certain that every self is aware that the other selves with whom it is in contact have their likes and dislikes also. To be conscious of a value-for-self relative to one's own likings is thus in principle to be conscious also of a value-for-others relative to others' likings. It must further become apparent at no very advanced level of reflection that likes and dislikes show a considerable amount of variation as between different persons, and that accordingly things which are values for A and B may very well be disvalues for the differently constituted persons C and D. But what must especially engage the attention of man as a member of a social group, of a body organised for a substantially common purpose, is not the values which are private to the individuals A and B and C and D, but the values that are *common* to all of them; the values which are values for *man*, rather than for this man or that man. Doubtless the 'manhood' or 'common human nature' to which reference is thus made will in early communities be interpreted exclusively in terms of the common human nature of the members of that community. But it is clear that this is a stage destined to be superseded with the advance of civilisation. The growing sense

of an universal human kinship can hardly fail to bring in its train a conscious interest in a human nature common to all men, and a consequent interest in the concept of a good which is good relatively not merely to the likings of this or that man, nor even to the likings of the typical man of this or that community, but rather good relatively to the likings of man as such, to the likings inherent in the common constitution of human nature.

It is, I think, altogether to be expected that when men have become '*kind-conscious*' the concept of '*value for man as such*' should evoke a very particular interest. Unlike the concept of value-for-self, it is conspicuously a concept of common interest, and thus an appropriate subject for the mutual interchange of ideas. What is good for the individual A is of great interest to A, but not as a rule of very much interest to individuals B, C and D. But what is good for man as man, in virtue of the common human nature shared by A, B, C and D alike, is a topic of interest to them all, a topic upon which they may pool their powers and their knowledge to mutual advantage. We might perhaps put it in this way, that mankind will have little interest in '*subjective*' goods *except* where the '*subject*' in question is the human self as such.

I think we may regard it as natural, then, that just as our value-consciousness is interested in value-for-self, conceived on the basis of the self's likes and dislikes, so also it should be interested in value-for-man, conceived on the basis of the likes and dislikes inherent in human nature as such. And it seems fairly obvious that substantially the same distinctions whose recognition sharpens, and at the same time gives depth to, the meaning of good-for-self are equally applicable to the conception of good-for-man. The distinctions of major from minor likings, of independent from dependent likings, of integral from partial likings, and of relatively permanent from sporadic likings, all retain their significance when it is the conative constitution of the human self as such, rather than of the individual historic self, that is under consideration: and in a way strictly analogous to that already discussed in the case of good-for-self, the appreciation of these distinctions must issue in the recognition of good-for-man as equivalent not to *any* object of liking to human nature, but rather to '*object of an independent, integral, and relatively permanent liking of human nature*'. Thus—to take a simple illustration which may help to elucidate the somewhat arid formula—knowledge would naturally come to be regarded as a good-for-man on the ground that man is so constituted that he has a liking for it which is entertained towards the object for

itself, is compatible with his liking nature as a whole, and is relatively enduring.

One distinction appealed to in the determination of the meaning of good-for-self is, however, not here in point, *viz.*, the distinction of present from prospective likings. It is not in point, because the meaning which human nature has for us at any time defines itself, as a matter of course, in terms of all that human nature has ever revealed itself to be, in its most ideally advanced quite as much as in its elementary manifestations. Accordingly, since the most developed human likings of which we have any conception will enter into what we mean by human nature, there will be no room here for a contrast between present and unawakened likings, appropriate as that distinction is to the case of a developing historic self.

As to the distinction of major from minor liking, it is not at first sight obvious whether the epithet 'major' ought not to be added to the qualifications of liking necessary to make an object of liking equivalent to 'good', both in the case of good-for-self and in the case of good-for-man. Strictly, I do not think it ought to appear. On the other hand, it seems clear that if anyone were attempting to draw up a list of 'goods-for-man' (to confine ourselves to this issue), he would probably not be satisfied to include in the list an object liked for itself, liked integrally, and liked in a relatively permanent way, if it should happen that it was not *also* liked *much*. Perhaps, however, the explanation is really a very simple one, just the fact that any attempt to draw up a list of goods will tend naturally to limit its field to the major representatives of the class. On the whole, I think we should say that the question of major or minor liking has no bearing on the *meaning* of goodness, but has a good deal of influence in determining what things we select as typical examples of 'goods'.

We have reached the point, then, of seeing the meaning of good-for-man, for a developed consciousness, to be an 'object of an independent, integral, and relatively permanent liking of human nature'. I do not mean, of course, that these several determining characters are explicitly present in the minds of all persons who use, even significantly use, the concept 'good-for-man'. But I do maintain that this formula expresses the meaning of the concept which we must suppose to have been operative in men's minds throughout the gradual process of determining what things are good for man, if that process has been a work of intelligence at all. This is the meaning, I believe, which has underlain and guided, whether explicitly recognised or not, the unsystematic reflections of the value-consciousness of generations of men,

reflections which have resulted in the now traditional acceptance by the civilised world of a more or less definite set of things as pre-eminently 'goods-for-man'.

But *are* there certain things that have received this public and traditional endorsement? The phrase 'good-for-man' is not a common phrase in ordinary discourse, and the critic may very reasonably ask what kind of value-judgment I have in mind when I speak of the value-consciousness of mankind having found expression in the endorsement of a definitive set of things as good-for-man. I answer—and here we come to the kernel of the matter—that the kind of judgment I have in mind is precisely the kind of judgment which is commonly supposed by the Objectivist to assert a good simple and unqualified, an 'intrinsic' good. When goodness is predicated of such things as knowledge or æsthetic experience, the Objectivist holds that we are using, or may be using, the term goodness in an absolutely simple, unrelational, unanalysable sense. I contend against this that the apparent simplicity is never a real simplicity; that actually there underlies the predication of goodness in all such cases the conception of a certain relationship between the things and the emotional nature of man as we know it, a relationship which is indispensable to the recognition of the things as good; and that this relationship is precisely expressed by saying that the things are objects of independent, integral, and relatively permanent liking to human nature.

This then is our hypothesis. And before considering the evidence in its favour it will be well to deal at once with an obvious objection. If people really mean 'good-for-man' in these judgments, why do they say just 'good'? The objection is, I think, less formidable than it appears. It seems appropriate, on reflection, that the relational qualification should tend to drop out where the relativity in question is to our common human nature. In the case of a quality which (as we claim to be the case with goodness) is naturally thought of as relative to persons in some sense, it will be necessary to add a relational phrase only where a *special* relativity to some *special* person or persons is intended. If *no* relativity is indicated, the natural presumption will be that the quality is relative not to any special party, but just to our common human nature. Hence the *omission* of the relational qualification may be said in such cases to serve exactly the same purpose as would be served by its *inclusion*. We might perhaps imagine a parallel case, to illustrate our point, in respect of colour-judgments. Even if we all believed, as many people do believe, that red is red only for man, we should continue to say

'this is red', not 'this is red-for-man'. We should only feel the need of appending a relational qualification if the relativity was to an *individual*: as, *e.g.*, a colour-blind person who was aware of his peculiarity might say 'this is red *to me*'. The omission of a relational qualification would imply that the relativity was merely to our common sensitive organisation.

Passing from this difficulty which meets us at the threshold, let us now consider the positive evidence that the formula I have reiterated does represent, in the sense of making fully explicit, what is really meant by good when that term is predicated in an ostensibly unqualified sense. The first and the chief point to which I wish to call attention is that the so-called 'intrinsic' goods of the Objectivist are precisely the kind of things that would come to be called just 'good' if our theory of the meaning given to goodness is correct. If goodness means the quality of being an object of independent, integral and relatively permanent liking to human nature, then the particular group of things which tradition has called just 'good', and which the Objectivist declares therefore to be intrinsically good, is exactly the group of things that we should expect. Truth and Beauty, for example, in their more philosophical dress as knowledge and æsthetic experience, fall into line at once. Each has in a pre-eminent degree the characteristic of being an object of independent, integral, relatively permanent,—and we may properly add here, major—liking of human nature. And the same thing may be said of such goods as health and friendship, on whose behalf the common value-consciousness of mankind has also made high, though less high, claims. Pleasure, I agree, occupies a somewhat equivocal position for our formula. It is an object of human liking, of major liking, of independent liking, and of relatively permanent liking. But whether or not it is an object of *integral* liking, a liking consistent with the liking nature as a whole, depends entirely upon what *kind* of pleasure it is. But then is not this equivocal position of pleasure for our formula precisely the kind of position it occupies for the value-consciousness of mankind also? The general attitude towards pleasure would probably be expressed fairly enough by saying that it is regarded as good in so far as it is not seriously obstructive of other values. But if so, that would admirably fit in with our formula. For it would mean that wherever the pleasure was of such a kind that it could be an object of integral liking to man it would be a good; and if not of such a kind, not good. But that is just what would be maintained if our formula were operative.

Now this applicability of our formula to the so-called intrinsic

goods is not, indeed, conclusive *proof* that our formula expresses what is really meant in these contexts by goodness. Nevertheless, I think we may say that if this is *not* what is meant by goodness, then the applicability of the formula is a very odd coincidence indeed. Actually, I think we are entitled to go further. I think we are entitled to say that the onus of proof now lies upon the Objectivist. For consider just where we stand. Our account started from a value-experience, admittedly subjectivist, which all must concede to be actual, and it proceeded from that basis to show, without any appeal to other than well-recognised psychical and other factors, how man would eventually come to pronounce as 'good', without explicit relational qualification, whatever was an object of independent, integral, and relatively permanent liking to human nature. We then found that the very things which are in *fact* pronounced to be good in this way—the so-called 'intrinsic goods'—possess all the characteristics of our formula. The Objectivist, on the other hand, has to appeal to an unique kind of perception whose claim to be something real is still, at best, *sub judice*, and whose strangely erratic behaviour even among its best friends is something of a scandal. Moreover, as was hinted earlier, the Objectivist is powerless to explain why so many mutually contradictory things have been called good in the same apparently unqualified way by different peoples in different ages—a phenomenon which is easily explicable on our view, since we can recognise the vast difference which different conditions of life and different levels of mental development must make to the things that are conceived to be objects of integral, independent and relatively permanent liking to human nature. It seems to me, therefore, that the onus of proof now lies upon the Objectivist, our present theory being one of greater *prima facie* probability. And I propose to devote most of what time remains to repelling objections to our theory, rather than to seeking for further positive support.

5.

It will not, I think, present itself as a very serious objection to anyone that our formula is certainly not explicitly present in the minds of most of those who predicate goodness (in the context we have been considering). It seems fairly clear that a formula can accurately represent what persons really mean by good even although they have no recognisable version of the formula before them. In ordinary discourse we are very seldom indeed conscious of the full definitory meaning of the complex terms that we use ;

and yet, in so far as we are using the terms intelligently, that meaning is operative in and controlling our usage. I am glad here to be able to enlist the authority of Dr. Ross. 'It appears', he writes, 'as if we cannot avoid recognising that there is such a thing as using a term which implicitly refers to a certain complex, while yet the complex is not explicitly present to our minds. And in principle this might, it seems, be true of good.'¹ After all, even in philosophical discourse, the valuable maxim 'define your terms' has to be applied with something less than absolute rigour if the argument is to advance at a tolerable pace at all.

But by what test, it may fairly be asked, are we to determine whether any particular complex, not explicitly before our minds when we use a term, is in fact implicitly present? Again I am well content with Dr. Ross's answer. 'If it is the correct definition', he holds, 'what should happen is that after a certain amount of attention to it we should be able to say "Yes, that is what I meant by 'good' all along, though I was not clearly conscious till now that it *was* what I meant."'¹ The process of criticising proposed definitions, he adds, has two moments. A definition of a term must be rejected if (a) we are able to point to things of which the term is predicable and the definition not, or *vice versa*; or if (b) even when the denotations of the term and of the definition coincide (or when we cannot be sure that they do not), we can 'see that a proposed definition does not express what we *mean* by the term to be defined'.¹ This seems to me to be a valid and valuable statement of the situation, and I should make no objection whatever to the application of such a test to the formula I have put forward as representing the meaning of 'good' in the value-judgments which 'at the first look' predicate intrinsic goodness. I am well aware, of course, that in Dr. Ross's judgment the application of the test proves fatal to *all* relational definitions of the nature of goodness. But while fully agreeing that it is fatal to the relational definitions which Dr. Ross actually cites, and which he appears to have alone before his mind, I must point out that our particular variety of relational definition does not appear in Dr. Ross's list at all—either explicitly or implicitly.

It will be worth our while, however, to consider with some care Dr. Ross's criticism of relational definitions of goodness, or at least of the subjectivist group of relational definitions. For, while we are claiming exemption for our own particular formula, it is evident that Dr. Ross intends his criticism to have an exhaustive reference. On pages 80-83 he furnishes what, I take it, he regards as a systematic classification of those theories of the

¹ *The Right and the Good*, p. 93.

nature of goodness which make goodness depend upon a relation to subjective or psychological factors. He arranges them under two main heads, *A* and *B*. *A* consists of those theories which hold that a thing's being good means that some person has, or some persons have, some kind of *feeling* towards it, *B* of those theories which hold that what is meant is rather that some person *thinks*, or some persons *think*, the thing to be good. Upon the second group, *B*, Dr. Ross wastes few words; and, being in perfect accord with all that he says, I propose to waste even fewer by making no further reference to this type of theory. But we must follow Dr. Ross into his sub-division of group *A*, to which our own theory would most naturally belong, and which Dr. Ross allows to possess a much greater *prima facie* plausibility than the other group. The sub-division adopted will be best explained in its author's own words. 'Theories of this type', he says, 'are divisible into those which identify goodness with the presence of some feeling (1) in at least one person, no matter who he is, (2) in the person who judges an object to be good, (3) in a majority of persons of some class or other—say persons belonging to a particular stage in the history of civilisation, (4) in a majority of mankind, or (5) in all mankind.'¹

Now of course the most conspicuous feature of Dr. Ross's classification from our point of view is that it omits altogether the particular variety of type *A* which seems to ourselves to offer the true definition. For it need scarcely be pointed out that being an object of liking to human nature is by no means identical with being an object of liking either to all mankind or to a majority of all mankind—while much less is it identical with any of the other suggested formulæ. Yet there is surely nothing unintelligible, or even strained, about the concept 'object of liking to *human nature*'. There are appropriate objects of liking to human nature just as there are appropriate objects to cat nature or dog nature. Cat nature is so constituted as to like stalking its prey and to dislike immersion in water. What is the difficulty about saying that human nature is so constituted as to like and dislike certain specific things also? Indeed, aren't we saying that kind of thing almost every day of our lives? And aren't our psychologists busily engaged at this very time in trying to ascertain just what the basic likes and dislikes of human nature are? And wasn't it the chief aim of the Greek moralist to determine what mode of life human nature was so constituted as in the end, and on the whole, to like best?

I hope, then, that no one will retort against me that while

¹ *The Right and the Good*, pp. 82-83.

Tom, Dick or Harry can have likings, it is not possible to assign likings to what is not a person but an abstraction, *viz.*, human nature. If we are to take that view in earnest, then we ought likewise to insist, I presume, that *instincts* cannot intelligibly be assigned to human nature either, since, strictly speaking, it is only an actual living creature that can have an instinct. But I fancy that the critic would wish neither to forbid other people to speak, nor himself to refrain from speaking, of 'the instincts of human nature'. In neither case is there any real difficulty about the meaning that is intended. Just as there are instincts which men have in virtue of their common human nature, so too there are likings which men have, or tend to come to have, in virtue of their common human nature.

But perhaps the best analogy for our usage is provided by the usage of the Greek philosophers in their search after man's *summum bonum*. Who is the 'man' whose *summum bonum* is sought? Not surely any particular man, but just 'man as such', the exemplar of our common human nature. The Greek moralist works with a *type man*, constituted by the conative, emotional, and intellectual proclivities believed to be common to human nature, sets him in a natural and social environment which, though inevitably relative to the age and place of the moralist, is made as little specific as possible, and seeks to determine what mode of life will afford the fullest satisfaction to a being so constituted and so conditioned. It is not anything essentially dissimilar, in my judgment, that mass opinion has been doing in the long process of constructing its list of 'goods for man'. The chief difference is due to the simple fact that in the one case the process is undertaken with scientific thoroughness and method, and in the other case not. That is why mass opinion is content with a set of pre-eminent *goods*, and does not concern itself with the deeper, and to ethical science vitally important, question of the relation of these goods to one another within the unity of *the good*. But so far as the concept of 'human nature' is concerned, the procedure seems fundamentally the same; and there seems no more difficulty in applying the concept in one case than in the other.

I must claim, then, that the subjectivist definition of good which I have placed before you is not to be put out of court on the score of being unintelligible, and I must insist that Dr. Ross's criticism of subjectivist theories is not exhaustive of the type so long as the classification upon which it is based ignores this particular variety. It *might* be the case, indeed, that the exclusion of it was only formal. That is, it might be the case that some of Dr. Ross's criticisms of the theories on his list are capable of

being adapted, with more or less trifling modifications, to the destruction of our theory too. It is therefore of first-rate importance to observe that this is not even remotely the case—as anyone may assure himself by even a cursory inspection of the relevant pages of Dr. Ross's book.¹ His criticisms simply do not touch our theory at all. And this is not really surprising. For in spite of superficial resemblances, there is one highly important difference between our theory and any of those which Dr. Ross considers. Each of Dr. Ross's theories makes the goodness of a thing depend upon the feelings (we may say, for convenience, the 'likings') of *some definitive existing person or persons*. Now it is extremely easy to show, as Dr. Ross does clearly show, that when we say 'X is good' we are not, as these theories would imply, meaning to assign to X a quality so impermanent that its coming to belong to X, and its ceasing to belong to X, are contingent upon the mere shift of favour on the part of some particular person or persons. There is undoubtedly an implication of permanence and 'objectivity' about our more 'absolute' value-judgments which belies any such description. But if goodness is made relative, as we make it, to the likings not of definitive persons but of human nature as such, this implication is saved. If what we mean when (in these value-judgments) we say 'X is good' is that X is related in a certain way to the liking nature of *man*, we are not implying, nor indeed even allowing, that X's possession of goodness is at the mercy of the changing likes and dislikes of any persons whatsoever.

I want to turn next—and, I may add, finally—to a difficulty of quite a different kind. It would, naturally, be a crushing objection to our theory, as to any other relational theory of the nature of goodness, if the Objectivist were able to exhibit to us *just one thing* (other, of course, than moral virtue) whose goodness is beyond question underived from relationship to subjective liking or to anything else. Has this ever been achieved? I am going to argue here that no Objectivist has even come near to achieving it. There are, of course, in the writings of all Objectivist axiologists chapters ostensibly directed to 'proving' some selected list of 'intrinsic' values. But close inspection will, I think, reveal a fatal flaw in these arguments. What we find in them is, as a rule, a very cogent demonstration that certain things are good irrespective of any relation to other *goods*, but it is merely *taken for granted*, on the basis of a prior attempted refutation in principle of subjectivist accounts of the nature of goodness, that these things are good irrespective of any relation to subjective

¹ *The Right and the Good*, pp. 83-84.

feelings *also*. Hence if the prior 'refutation' be itself fallacious (as we have seen reason to believe must be maintained in Dr. Ross's case), the proof of the intrinsic value of particular good things will be fundamentally defective.

I shall illustrate once again by a reference to Dr. Ross's stimulating book. In his short chapter upon 'What things are good' Dr. Ross claims to be offering considerations which will assist the reader to apprehend virtue and knowledge and certain other things as each having a value absolutely in and for itself. What the reader in fact finds is that his arguments are one and all designed to demonstrate that the goodness of each of these goods is independent of any relationship to other goods, but that nothing whatsoever is done to show that the goodness in question is independent of a relationship to subjective feelings. The latter question is not, I think, affected one way or the other by a single word in the whole chapter. We may take the thoroughly typical argument whereby Dr. Ross seeks to persuade us of the intrinsic value of knowledge. He asks us to 'suppose two states of the universe equal in respect of virtue and of pleasure and of the allocation of pleasure to the virtuous, but such that the persons in the one have a far greater understanding of the nature and laws of the universe than those in the other. Can any one doubt', he goes on, 'that the first would be a *better* state of the universe?' Dr. Ross expects, and rightly expects, that this question will receive an affirmative answer. But I must point out that an affirmative answer does not carry with it a recognition of *intrinsic* value in knowledge. Dr. Ross has so constructed his hypothetical situation that an affirmative answer certainly entails the recognition that knowledge has a value which is not dependent upon a relationship to other *goods*. But, so far as his argument here is concerned, it must remain an entirely open question whether the value recognised is an 'objective' quality, or whether, on the other hand, it is dependent upon a relationship to subjective factors—such as, *e.g.*, the likings of human nature. The terms of Dr. Ross's argument are such that no light at all can be shed upon this crucial issue.

How, one might ask, would Dr. Ross's argument require to be supplemented in order to become relevant to the question of the *objectivity* of the value of knowledge? It would, I think, be formally satisfactory if, in being invited to appraise the relative value of these two states of the universe, we were at the same time instructed to rule out from our minds all considerations arising from our familiarity with human likes and dislikes. Thus we

¹ *The Right and the Good*, p. 133.

should be obliged to suppose, for the sake of the argument, that there is no native impulse of curiosity in man which makes him come to like knowing for its own sake. If, fulfilling these conditions, we were to proceed to put to ourselves Dr. Ross's question, an affirmative answer *would*, I think, imply the recognition of a strictly 'intrinsic' value in knowledge.

But *should* we then be able to return an affirmative answer? It is a matter which each must decide for himself by personal experiment, but I feel convinced that, if the experiment be performed with due observance of the conditions, only a negative answer will be found possible. No legitimate ground remains, I believe, for judging the first state of the universe to be the better. Old emotional habits, like old cognitive habits, die hard, and there is undoubtedly a great mass of prepossessions to be broken through before one can hope to return a fair answer. But I must leave the experiment with the reader, pausing only to draw attention to two pit-falls which seem especially liable to engulf the unwary. (1) We know that knowledge, even if it were not itself liked, would still be instrumental to a host of things that are liked, and this makes it difficult for us not to think of the state of the universe with knowledge as the 'better' state. But it is clear that the terms of the hypothesis make this consideration irrelevant. Reference to other goods, whether as 'objects of liking' or in any other sense, is definitely ruled out. (2) A good many of us are more deeply influenced than we are apt to realise by an inherited religious tradition which leads us to think of our faculties as given to us by God for use and development, so that it is in accord with the Will of God, and so far 'good', that they should be exercised to the full. On this ground alone there is a powerful disposition in most of us to regard a state of the universe in which the faculty of knowledge finds active expression as better than a state in which it does not. Evidently, however, we are not, in making this judgment, recognising knowledge to be something 'good in itself'. We are merely recognising respect for the gifts of God, or obedience to the Divine Will, to be a duty.¹

¹ It must be admitted that it is extremely difficult, in an ideal experiment such as that which we are here called upon to perform, to prevent one's value-judgments from being affected by the cross-currents of moral and religious duty. Many, perhaps most, educated persons, even apart from religious considerations, believe it to be their duty to develop their capacity for knowledge. On that account the conception of 'knowledge' is closely associated in their minds with the conception of 'goodness'. But clearly the effort has got to be made to abstract from the influence of this connection, if we are seeking to discover, by the device of experiment upon our value-responses, whether knowledge is good simply *as such*, in and by itself, as the Objectivist claims that it is.

It appears to me, then, that Dr. Ross totally fails to demonstrate the objective goodness of his 'intrinsic goods', and I am not able to conceive any other method likely to yield a different result. Dr. Moore's attempt fails for exactly the same reason as Dr. Ross's. His argument in Chapter VI. of *Principia Ethica* depends essentially upon a prior supposed refutation of relational theories of the meaning of good in Chapter I.; and, as we noted earlier, Dr. Moore is himself the latest recruit to the ranks of those who find the reasoning of Chapter I. fallacious. I do not find, therefore, in Dr. Moore's attempted demonstration of intrinsic goods anything which places in serious jeopardy the central contention of this paper—the contention that there is nothing whatsoever, with the single exception of moral virtue, which does not derive its goodness from a relationship to subjective liking.

6.

It is more than time that this paper drew to its belated close. Nevertheless, I must add just a very few brief words upon a matter alluded to at an early stage of the paper, if I am to round off my theory with any pretence of completeness at all. It has been an implication of my argument that when we predicate value of morally virtuous conduct we mean something rather radically different by the term value from what we mean when we predicate value of anything else whatsoever. In the latter applications the meaning always involves an essential relation to human liking. In the former application no such reference is involved. Yet there clearly must be some common factor in the two meanings. Otherwise, why use the same term 'good' or 'valuable' in both cases? What is this common factor? What is the analogy between the usages which justifies us in employing common terms?

The correct answer, I think, is that all usages of the term 'good' signify at least this common feature in that to which goodness is attributed, *viz.*, that it is the object of what may perhaps least misleadingly be called a *pro*-attitude: just as that to which badness is attributed is always the object of a *contra*-attitude. An object of liking is quite obviously the object of a *pro*-attitude. But it is equally obvious, when we reflect upon it, that morally virtuous conduct is likewise the object of a *pro*-attitude. The latter *pro*-attitude is certainly not the same kind of *pro*-attitude as is entertained towards an object of mere liking. But a *pro*-attitude it undoubtedly is. The identity and the

difference can probably be made most plainly apparent by reflecting upon the value-judgments ingredient in any simple case of so-called 'moral temptation', in which the course we believe that we ought to follow is recognised to be incompatible with the course that we like best. Our mental attitudes towards the two courses are conspicuously different, but both of them are beyond question *pro*-attitudes. Indeed, if they were not, there could be no consciousness of inner conflict. It is equally certain, on the one hand, that the morally right course does not appeal to our mere 'liking', and, on the other hand, that it does *appeal* to us. Its appeal is such, indeed, that it may be made by us the motive of our act, and thus be adopted as our 'end' in preference to that which we 'like best'.

If this is true, it appears that what we mean ultimately when we predicate goodness of moral virtue is that it is an object of approval or favour to the moral consciousness. Does this then imply, it may perhaps be asked, that even the goodness of moral virtue is in the last resort 'subjective', consisting in a certain relationship to a state of consciousness? It is, I think, partly a question of the use of words. Most people, however, would probably agree that the moral consciousness, though it must be *in* a subject, is yet not 'subjective' in the same sense as desires and likings are 'subjective'. But I cannot now embark upon the long and arduous task of defining the true status within the self—much less within the whole scheme of things—of the moral consciousness. A fully adequate theory of value could not, I am sure, be dispensed from this obligation. But the pretensions of the present paper are more modest. I am content if I have established a *prima facie* case for the view that the meaning of value as applied to all the so-called intrinsic values with the exception of moral virtue involves an essential relationship to human liking.

II.—THE PREDICAMENT OF NATURALISTIC EMPIRICISM.

BY H. D. ROELOFS.

THROUGHOUT the history of philosophy we find specific movements claiming in some pre-eminent sense the title, Empirical. This claim, if it has any significance whatever, must depend upon a contrast. In one age the contrast is between sense and intellect, a philosophy based upon the former being said to be empirical, a philosophy based upon intellect, not. In this there is also the pretension that the empirical philosophy is founded upon fact; and the implication that the so-called non-empirical philosophies derive from fictions, fancies, prejudices. Yet in the generic sense of the term, all philosophies must be empirical, for in the end there is nothing from which they can spring except experience. Every philosophy, then, that makes a special point of its own empiricism, must in fact be a qualified empiricism. It is that qualification which gives to such philosophies whatever uniqueness they possess and determines in the end their adequacy or inadequacy to fact. That this is true does not mean it is well recognized. Quite the contrary. Claims are more readily stated and far more readily understood than the evidence which can be brought to their support. The word, especially if it be boldly spoken, is only too often taken for the deed, though the deed may be lacking. Hence it is the perennial business of philosophical criticism to question claims, to search for and examine deeds.

In our own time the philosophy which is most resolute in its claim to be Empirical without qualification is called variously Pragmatism, Instrumentalism, Naturalistic Empiricism. Like its historical predecessors, however, its claim to be empirical rests upon a qualifying contrast. In this case the contrast is not between sense and intellect, but between one method and all others. Pragmatism calls its own method the Empirical, the Experimental Method. All others, by this easy use of an adjective, are imputed to be not empirical, the philosophies using these other methods, not based on experience. This Empirical Method, we are further told, is the adoption and adaptation for philosophy of those methodological procedures which have been

developed by the natural sciences and whose success in the original fields of their application have proved their excellence. Thus at a stroke two claims are consolidated. Instrumentalism presents itself to the public as at once the philosophy and the only philosophy which is both empirical and scientific. The prestige thus obtained for Instrumentalism is very great. Yet prestige is not itself conclusive evidence. The proper question to put to this philosophy is this: Does its method actually provide that empirical character, that sure foundation in fact, that adequacy to fact, which is claimed?

I intend to investigate this question in a single, crucial case. I find it in the place and treatment given by Naturalistic Empiricism to feeling—that this is a crucial case must, of course, be part of the exposition. And I think it can be proved that the outcome is a destructive dilemma. If Naturalistic Empiricism holds exclusively to its chosen method, the treatment of feeling is at variance with the facts; if feelings are treated empirically, the empirical method, so called, is abandoned. This is the predicament of that philosophy.

As might be expected, and quite properly, I shall take my material from the writings of John Dewey. He is fond of the truism that what is eventual is determined by what is taken to be primary. Let us apply this truism to his own philosophy. For him, along with the rest of us, the Cartesian dualism is a dominating inheritance. If we look at that dualism with the eyes of an Instrumentalist, we discover certain specific centres of trouble. In the Cartesian world, omitting God, there are minds, matter, and knowledge. The *res extensa* is the chief object of knowledge and is known by the minds. But for the Instrumentalist these minds are in the Cartesian philosophy impotent as regards the *res extensa*. They know, but they do not do. Though their knowledge is said to be true, it accomplishes nothing. All the overt—mark the word—all the overt doing is restricted to bodies enmeshed in the mechanical *res extensa*. There are secondary qualities, too, in the Cartesian world, and purposes, feelings, beauty, ugliness, good and evil. But they have place only in the *res cogitans*. In the *res extensa* they neither exist nor count. This is that separation of knowing and doing of which Dewey so frequently complains. Finally, although a Cartesian will insist upon the reality of minds, knowledge, feelings and the rest, not one of these is directly discoverable by what is now called scientific method, the method of objective, multiple observation and experiment. That method discovers only the *res extensa*. Such, in brief, are the difficulties.

We now turn to needs. Dewey is a philosopher. He must have mind. More, Dewey is a moralist, a social reformer. Therefore feelings, purposes, good and evil, beauty and ugliness, and all secondary and tertiary qualities inextricably bound up with these, must be real. Finally, knowledge must possess an effective functional relation to objective reality, to the end that knowledge may further social reform and the enjoyment of natural goods.

The identifying characteristic of Naturalistic Empiricism, and in its own estimation its sovereign virtue, is its claim to satisfy these needs and to rectify the defects it finds in Cartesianism by the exclusive use of the empirical, the scientific method. The primacy of this method is absolute. All that is discovered and verified by this method is knowledge. What is not so discovered is not knowledge, what is not so discoverable, is not knowable. Apparently opposed to this is the fact that in classic Cartesianism the whole realm of the *res cogitans* was neither discovered nor known by that kind of empirical method. Yet Pragmatism must have this realm. Accordingly, to get what he needs by the sole method he accepts, is the primary metaphysical problem of Dewey's philosophy.

How is this problem to be solved ? By going to experience, of course. Science went and so will Dewey. What does he find ? Let us take the answer in his own words. "Experience is *of* as well as *in* nature. It is not experience which is experienced, but nature—stones, plants, animals, diseases, health, temperature, electricity, and so on." (*Experience and Nature*, p. 42.) "Empirically, things are poignant tragic, beautiful, humorous, settled, disturbed, comfortable, annoying, harsh, consoling, splendid, fearful ; are such immediately and in their own right and behalf. . . . These traits stand in themselves on precisely the same level as colors, sounds, qualities of contact, taste and smell. *Any* quality as such is final ; it is at once initial and terminal ; just what it is as it exists." (*Ibid.*, p. 96.) "Human experience in the large, in its coarse and conspicuous features, has for one of its most striking features preoccupation with direct enjoyment." (*Ibid.*, p. 78.) ". . . in every event there is something obdurate, self-sufficient, wholly immediate, neither a relation nor an element in a relational whole, but terminal and exclusive. [Every event has] those ineducible, infinitely plural, undefinable and indescribable qualities which a thing must have in order to be, and in order to be capable of becoming the subject of relations and a theme of discourse. Immediacy of existence is ineffable. But there is nothing mystical about such ineffability ; it expresses the

fact that of direct existence it is futile to say anything to one's self and impossible to say anything to another. Discourse can but intimate connections which if followed out may lead one to *have* an existence. Things in their immediacy are unknown and unknowable, not because they are remote or behind some impenetrable veil of sensation of (*sic* !) ideas, but because knowledge has no concern with them. For knowledge is a memorandum of conditions of their appearances, concerned, that is, with sequences, co-existences, relations. Immediate things may be *pointed to* by words but not described or defined. Description, when it occurs, is but a part of a circuitous method of pointing or denoting ; index to a starting point and road which, if taken, may lead to a direct and ineffable presence. To the empirical thinker, immediate enjoyment and suffering are the conclusive exhibition and evidence that nature has its finalities as well as its relationships." (*Ibid.*, pp. 85, 86. Italics Dewey's.)

Together these make a long passage to quote, but it is essential that we have all the material before us at once. The first step of the solution is the simple assertion that in experienced nature exist all the qualities Dewey needs. If the reality of tertiary qualities, let alone secondary, can be proved as easily as that, and is that obvious, surely it is odd that the multitudinous empirical investigations of science have not had more to say about such qualities. How is it that ordinary experimental investigations often end in the actual denial of the reality *in nature* of colours and sounds, of beauty and ugliness ? The second stage of Dewey's solution is to overcome this difficulty. A distinction is made. All these qualities are empirically *real*, but they are *not* empirically *known*. Nor are they known in any other fashion—there is no other. Quite the contrary. These qualities are all qualities of *immediate* existence and "things in their immediacy are unknown and unknowable". It is a neat distinction, but not all we have to learn. If this sundering of knowledge and the qualities of existence were left unbridged, cognition would be condemned to a worse impotency than that from which it suffers in Cartesianism. So we are offered a new account of the nature and function of knowledge. There is and can be no knowledge of the qualities of immediate existence, but there can be knowledge of *how to reach* those qualities. Knowledge has nothing to do with the nature and qualities of what exists, but only with the conditions of existence, with the "road which, if taken, may lead to a direct and ineffable presence". This is the famous instrumentalist theory of knowledge, too well known by this time to need further exposition. Our concern is with the end of the road.

If knowledge is confined to the route and cannot grasp what is at the end of it, what does enable us not merely to travel hopefully towards reality but actually to arrive ? Direct enjoyment is the answer, having, undergoing, suffering—in a word, feeling.

In feeling we grasp reality as it is, tragic, humorous, beautiful, ugly, and all the rest. Though never known, of these qualities it can somehow be said that they exist *in experience*, therefore their reality is established. For the doubter specific directions can be given for reaching this particular quality and that, so that their reality can be verified. If anyone still asks, do I not need to be able to *know what* I enjoy or suffer in order to verify that I am having precisely the ineffable experience promised by the directions and none other ?—the answer must be, you are asking to know the unknowable, which is certainly foolish, and you are evidently not content with enjoying what you have, which is ungrateful.

From the foregoing the crucial rôle of feeling in Dewey's philosophy should be evident. If it has often been overlooked, that is because the presentation of that philosophy has focussed our attention elsewhere. Confronted with the scientific account of nature, our attention has been focussed on the problem of how to accept both scientific method and its results, and at the same time avoid the conclusion that all secondary and tertiary qualities, and all values, are merely subjective. In the name of empiricism and scientific method Dewey tells us that our problem is solved by a correct apprehension of the nature and function of knowledge. Its business is *not* to tell us *what* nature is, but only how to move about in nature. When we do so move, we shall find *in nature* all those qualities and values for whose objective reality we have been so concerned. There is so much truth in this that few readers have stopped to ask Dewey how he *knows* nature is tragic, beautiful, and so on. If they did, the answer no doubt would be, because nature is thus experienced. But this is no answer to that question, as becomes apparent as soon as one recalls what has been said of the limits of knowledge. Recall the quotation beginning, "Empirically, things are poignant, tragic, beautiful . . ." (*supra*, p. 302), and at the same time remember that not one of these aspects of things is either known or knowable. They are only "enjoyed".

Since feeling grasps what cognition cannot reach, we may well ask what feeling is and how we know it. Whatever answer Dewey makes must conform to two separate criteria. The one is the actual nature of feeling—the answer must accord with the facts. The other is the central principles of Dewey's philosophy

—the answer must be consistent with them. Of his principles, those most relevant to this question are the exclusive reliance on his version of empirical method, and the rejection of that kind of immediate cognition usually called introspection. Feeling must itself be objective, and be discoverable and describable by the usual procedures of objective observation and experiment.

The ordinary person thinks of feeling as something happening to and belonging to himself. He assumes he knows both what feeling itself is and what he feels, through his immediate cognition of the content of his own consciousness. He often assumes further that in feeling he comes to know a quality of something objective. Take the common remark, how cold the wind feels. Here the ordinary person assumes he knows both the content of his feeling, the sensation of cold, and that quality of the wind which he says he feels. The ordinary person assumes that somehow both the subjective order and the objective are within his cognitive grasp. Yet there is one thing he does not do. He does not assign to the *wind* the feeling he himself feels. The wind is said to be cold, to possess that quality, but that quality in the wind is not a feeling. The feeling belongs to the experiencing subject. A quality but not a feeling is what is objective. Yet an ordinary expression such as how cold the wind feels, is sufficiently ambiguous to hide these distinctions. And that gives Dewey the opportunity to begin his objectification of feeling in a fairly plausible fashion.

It is instructive to go back over our quotations and observe that in no case does Dewey provide a subject for experience, and that in describing what is experienced he uses terms ambiguously available to denote both the feeling states of self-conscious beings and qualities of things. He does not say '*our* experience is of nature', or '*we* enjoy and suffer', but "experience is of nature", and "human experience [is preoccupied] with direct enjoyment" (*supra*, p. 302). And in describing nature he writes, "Empirically, *things* are poignant, tragic, . . . disturbed, comfortable, . . ." (*supra*, p. 302. My italics). This is deliberate. Just how deliberate we can judge from Dewey's own definition of feeling. "'Feeling' is in general a name for the newly actualized quality acquired by events previously occurring upon a physical level, when these events come into more extensive and delicate relationships of interaction. More specifically, it is a name for the coming to existence of those ultimate differences in affairs which mark them off from one another and give them discreteness; differences which upon the physical plane can be spoken of only in anticipation of subsequent realization, or in terms of different

numerical formulæ, and different space-time positions and contingencies. Thus qualities characteristic of sentiency are qualities of cosmic events." (*Experience and Nature*, p. 267.)

Assuming for the moment that we know what feelings are in the ordinary way and in the ordinary sense of the term, and not inquiring whether Dewey's definition is accurate, let us examine this definition to discover its essentials. I list these in the order of my subsequent consideration. Feelings are :

1. Ultimate.
2. Qualities.
3. The reference or locus of these qualities is first, in something quite general, *events*, and then, *more specifically*, in something *still more general* and *vague, affairs*.
4. The function of feelings is to mark off one affair from another and make affairs so qualified, *discrete*.
5. The origin or source of feelings is the coming of physical events "into more extensive and delicate relationships of interaction".

The last sentence in the quotation—"Thus qualities characteristic of sentiency are qualities of cosmic events"—does not give a new element in the definition. The sentence is in fact taken from the next paragraph in *Experience and Nature*. I quote the sentence, however, because it gives clear and emphatic restatement to what I list as the third element in the definition. Feelings as defined, though characteristic of sentiency, are qualities "of cosmic events", no less.

The first element in the definition creates no difficulty. That feelings are ultimate is a commonplace, if by ultimate is meant that their essence is ultimate for knowledge. The causes or conditions of a specific feeling may be investigated, also its consequences. But precisely *what* that feeling is, can be *known*, if at all, only by actually experiencing it. It is in that sense that feelings are ultimate. The second element, however, is both ambiguous and transitional. If we say feelings are qualities and mean qualities of the experience of a conscious individual, we have one meaning, understandable though not stated in the usual terms. It is not Dewey's meaning, however. When Dewey makes feelings qualities, this is transitional to making feelings objective rather than subjective. It is really the rhetorical or poetic meaning of feeling. Webster's dictionary supplies good evidence here. Under "feeling, n." we find, "7. State or quality of that which causes or expresses feeling conceived as embodying the feeling; objectified feeling; as, the *feeling* of a tomb, of a picture

... 9. That quality of a work of art which embodies the emotion of the artist, and is calculated to affect similarly the spectator." "9" is obviously a variant of "7". These two usages are the only ones whose definition uses the term "quality", and both refer to feeling in the more usual sense of an experience or state of a *subject*—"objectified feeling", "embodies the emotion of the artist"—and the meaning of feeling as quality is understandable only through this reference. This is of the highest importance. The point involved is stated with such precision by the dictionary that I will quote further. "8. Psychology. In the broadest sense [feeling is] a state of consciousness, or consciousness in general considered in itself and *apart from any reference to an object of perception or of thought.*" (My italics.) In standard usage feeling refers primarily, even exclusively, to a *subject*. The reference of feeling to an *object* is derivative, secondary, understandable only in terms of the primary reference. It imputes to an object what originates and belongs in its proper nature to a subject. But Dewey makes what is derivative, primary and independent. This second element in his definition marks his departure from ordinary usage. Where does he go?

Just as the second element in the definition is critically transitional, the third is conclusive. Feelings are qualities of affairs. I wish to emphasize the coherence and importance of this element in its relation to Dewey's philosophy in general. Making feelings qualities of affairs establishes feelings as objective, matters of empirical observation, *real*. Feelings as ordinarily understood are discovered only subjectively. Only introspection can examine them. This, for Dewey, is an insuperable obstacle to their discoverable reality. Hence he must define them as something else, and he does. His definition is effective in accomplishing what he must accomplish.

In the same sense the fourth element, that which names the function of feelings, is also effective. It identifies feelings with those qualities found in immediate experience which give to that experience uniqueness, the flavour of individuality, the infinite ranges of enjoyment and suffering. The fifth element completes what Dewey requires. The statement that feelings come into existence when "events previously occurring on a physical level ... come into more extensive and delicate relationships of interaction" may seem mere nonsense if we are looking for an exposition of what feelings actually are. But that is not the function of this statement. The suggestion it makes that feelings are not physical but something beyond is only a minor detail. A coherent detail, however, for it fits in with Dewey's general

account of mind as emergent. *The function of this fifth element in the definition is to connect the occurrence of feelings with those interactions and relations studied by science and thus provide the basis for the controlled production of feelings by scientific means. Feelings are terminations, endings. As such they are not only found in the ordinary course of nature, but because of the conditions of their origin, they can be produced by and under the control of those instruments Dewey calls knowledge. Feelings thus produced are indeed, as we have already learned, the proper termination and consummation of every application of knowledge. The "return to crude experience" is the return to feeling.*

Some may find a difficulty in this assertion of the objectivity of feelings. How is it, they might ask, if feelings are objective, open to empirical observation, they are never mentioned, never, seemingly, discovered by natural science? This is no difficulty. We have already had this explained. Feelings are qualities. Therefore, they are real but ineffable, discoverable but not knowable.

From the standpoint of Dewey's philosophy there is no error in this definition of feelings. To the uninitiated it is no doubt a difficult, complex, and perplexing definition. Taken in its setting and rightly understood, it is none the less a masterpiece of consistent construction. It defines feelings with admirable precision as *they must be for Dewey's philosophy*. But does it define feelings? That is the final, the deciding question. It is worse than quibbling, it is positively and perniciously evasive, to say that Dewey surely can define his terms in his own way. This is not a question of terms. It is a question of empirical fact. Not even an Instrumentalist will deny the existence of feelings independent of what any philosopher may say of them. Dewey's definition, anyone's definition, can be at best a *definitive description* of what feelings independently of that definition essentially are. The definition cannot claim for itself ontological creative power. The question is legitimate, is Dewey's definition a definition of *feelings*?

I have what I regard as instructive and conclusive evidence on this matter. I obtained this evidence as the result of an experiment. I made copies of Dewey's definition of feeling with the word "feelings" itself left out and the symbol "X" substituted. "X is in general a name . . ." etc. I then sent these copies to a number of men who fulfilled the following qualifications. Each is a professional philosopher or psychologist of recognized competence. Each has read John Dewey and in particular *Experience and Nature*, the book from which the quotation is

taken. At least two of these men are usually regarded as professed Instrumentalists. As to the classification of the others I am uncertain. I assumed, further, that each in the ordinary sense had experienced feelings and knew, again in the ordinary sense, what they are. I had previously ascertained, again by an experiment, using definitions of common terms as given in Webster's dictionary, including the term "feeling", that given such definitions with "X" substituted for the term defined, it is possible to guess the term itself. In this control experiment there were no failures. The questions I asked of my subjects with reference to the quotation from Dewey were these :

1. Can you identify the quotation ?
2. If not, can you give the term of which the above is a definition ?
3. Having opened the enclosed envelope and read the term thus defined, do you now consider the definition adequate or inadequate, correct or incorrect. Please explain.

Had I the space I would give the results of this experiment in their entirety, for they are illuminating and often amusing. But a summary is all we need.

Only one person named the right author, Dewey, with assurance. His reason for this identification is worth giving. It was the use of the term "affairs".

Not one guessed the term itself. "Individuality", "sense quality", "awareness of qualitative individuality", "mind", "the psychical" are some of the answers. These are "warm", as we say, but the differentiating phrase, "upon the physical level", in the definition points definitely in the direction of the psychical.

Far more important than these answers are the reasons given. Several men named Dewey as possibly the author, along with one or two other possibilities which, in fairness, I must suppress. For the reason given for these guesses was invariably the "badness", the "unintelligibility", of the definition. It is not then surprising that the answers to the third question were unfavourable. One subject did reply that the definition in his opinion correctly "denotes the locus of 'feeling', taken genetically, in experience, and in a very vague and obscure way, defines the type of events characterized by 'feeling'". He also added it was "hopelessly Deweyish". The others rejected the definition outright as a definition of "feeling", at times in no uncertain terms. "Non-sense", said one, himself a well-known member of the Dewey school. "A characteristic studied evasion of the issue", said another, not an Instrumentalist.

What was further said I found informative as to what the writers thought about "feelings", but we now have the evidence directly relevant to our question, is Dewey's definition a definition of "feeling"? I wish to call attention to an apparent discrepancy between what I said of the definition and what the subjects of my experiment said. It was and is my opinion that the definition is "a masterpiece of consistent construction" (*supra*, p. 308). They condemned it as "nonsense", "hopelessly Deweyish", "a studied evasion". Yet there is really no discrepancy, no contradiction. Rather there is agreement. The two sets of opinions are seen to be in agreement when we make plain the basis of each. In my own examination of the definition it was known that feeling is the term to be defined, further it was assumed that we all know what feelings are, and the question was ruled out for the time being whether the definition actually defines feelings or not. The judgment as to the excellence of the definition depended upon taking it exclusively as a definition of what *feelings must be in Dewey's philosophy*. In that there is not the slightest implication one way or the other that feelings empirically are what Dewey says they are.

My subjects, however, considered the definition from two other points of view. Given the definition but not knowing what it was intended to define, they first tried to determine what it did define. In that effort they failed and they were not hesitant in stating what they thought was the reason for their failure. They found the definition unintelligible—it did not define anything *within their ordinary experience*. My control experiment should be remembered. Dictionary definitions do define in that sense, and their terms can be determined from the definitions, provided the things named by those terms are within the common experience of the subjects of the experiment. Now certainly feelings are within our common experience. Yet Dewey's definition is unrecognizable as a description of them. The conclusion is plain: what Dewey offers as a definitive description of something within our common experience, is no such thing. What is it then? It is a theoretical construction of something Dewey needs in his peculiar philosophy.

To return again to my experiment, having failed to determine the term alleged to have been defined, the subjects were then informed what that term was and asked to reconsider the definition from this specific point of view, is this a definition of feeling? To this question their answers, with the qualified exception noted above, were "no". This resolves the apparent discrepancy between my original estimate of the worth of the definition and

theirs. I held that the definition defines feelings as they must be in Dewey's philosophy. They assert it does not define feelings. I agree. As a definition of feelings, of actual, empirical, real feelings, what Dewey says is not in accord with experience. Early in this paper it was emphasized that the prime question to put to Dewey's philosophy is this: Is what you say empirical? In this matter of feelings, we now have that question answered.

It is not enough, however, in philosophical criticism to accuse another philosopher of being wrong in fact and supplying evidence to that effect. There must be explanation, if that be possible. Why did the subjects of the experiment reject the definition? Wrote one, "there is no recognition of a feeling or a sensation as being such, save by a self-conscious inquirer and knower." This squares with common sense, with the dictionary, with the facts. But it is precisely what Dewey cannot admit.

This omission of reference to a self-conscious subject, is the chief factor in preventing Dewey's definition from being in accord with the facts. In an extremely *general* way the description of "feeling" as coming into existence when events "come into more extensive and delicate relationships of interaction" is correct. That is, the correlation of the occurrence of feelings with "interaction" is probably sound. But such a statement tells us practically nothing about feelings. For it is equally true of the occurrence of noises, earthquakes, and the recording of an earthquake by a seismograph. Nor are we helped by the qualifying adjectives, "more extensive and delicate". Of the three interactions just mentioned, which is the "more extensive and delicate", the noise, the earthquake, or the recording of the earthquake by the seismograph? Not one, certainly, is a feeling. Is it because the interaction is not extensive or delicate enough? I have not forgotten that these interactions are also differentiated from those on the physical level. But we must note also that it is the *same* events which are to acquire a new quality, feeling, when these same events "come into more extensive and delicate relationships of interaction". It is the entering into those relationships, the more extensive and delicate, which both differentiate these events from being merely on the physical level, and give to these events the new quality, feeling. Therefore, if an earthquake is an event on the physical level, it remains on the physical level and lacks feeling only because its interactions are deficient in extensiveness or delicacy, or both. Earthquakes are surely extensive enough in their interactions, so it must be lack of delicacy. Of course that is it, for is it not customary to identify lack of delicacy with lack of feeling?

But we are not done with the definition. Perhaps the second part beginning "More specifically" will help us. This part tells us the function of feelings. Their function is to be "those ultimate differences in affairs which mark them off from one another and give them discreteness". It is granted that certain qualities do this. But are those qualities feelings? One of the subjects of my experiment answered that question for us. Take two batches of bread, is his argument. Let one become "heavy" bread, the other "light". These two qualities are the result of interactions into which the two batches of bread entered. They also are ultimate as differentiating the two batches. These interactions are presumably extensive and delicate. But are the resulting qualities feelings? Who would seriously contend they are?

"Heavy" and "light" are qualities, however, and the main result of Dewey's definition of feelings is to identify them with qualities of objects or events. That is where the definition breaks with the facts. For while we may, if we like, call feelings, qualities, they are qualities not of objects *as objects*, but of *subjects*. A man is an object and he has feelings. But he has feelings not as an object but as a subject. Loaves of bread and earthquakes have qualities but they do not have feelings. Why? Because they are not subjects.

This is so obvious that one may well be puzzled over Dewey's not only ignoring it but by implication denying it. The explanation is to be found exactly where Dewey himself would tell us to look were he criticizing this sort of denial of plain facts. We must look to Dewey's initial problem and the initial factors he *selects* for his solution. His problem is the general bifurcation of reality inherited from Descartes and made intolerable by the development of science. The particular aspect of that problem that most concerns us is the bifurcation of what is real and what is valuable. The relevant initial selection of factors within which a solution is to be found is his acceptance of the objective empirical method as not only the one source of genuine knowledge but the one route to what is real. The application of this method to qualities in general was successful only at the cost of asserting that empirically discovered qualities are real but *not knowable*. The reality of feelings can be established only by a similar method. If feelings are unknowable they are no worse off than qualities in general, but how can they be shown to be real? Only by making feelings qualities, qualities of objects or events just as all other qualities are. Only then can they be discovered by objective methods. This is readily proved. Can I discover and verify by

objective, empirical methods, including physical experiments and physical apparatus of any and every sort, the actual existence of your *feelings*? I am not even asking whether I can in this fashion *know what* your feelings are, but only can I discover whether you have any?

The answer is notorious. I cannot. The existence of feelings is directly certain only to a subject in and as he has them. We can go further, he can *know* that feelings exist *in the first instance* only as he *knows* his own feelings. For this a self-conscious being capable of immediate knowledge is required. Only such a one can intelligibly say, "I am aware of my own feelings and know both that I have them and what they are."

It is illuminating to consider the treatment of exactly this issue in the latest attempt by a Pragmatist to make good the reality of feelings. Prof. Brown takes as his illustration the feeling-quality pain. "If one could reproduce the exact conditions of structure of an aching tooth by chemical synthesis in the laboratory, it would be necessary to say it had a pain-quality although nobody felt it, in accordance with the postulate of qualitative structural concurrence." (H. C. Brown, "Mind—An Event in Physical Nature," *Phil. Review*, vol. 42, No. 2, March, 1933, p. 141.) In our observations on this statement we can waive any question as to the precise meaning of "the *exact* conditions of structure of an *aching* tooth" (my italics), *i.e.*, as to whether this would not have to include a conscious subject to be fully "exact". We need to examine only the statement that it would be *necessary* to assert the existence of the pain-quality—why? "In accordance with the postulate of qualitative structural concurrence". Granted. It would indeed be necessary. But is it justified? Would there ever be any *evidence* of the existence of *feelings*, unless somebody felt them and, in feeling, knew *that* and *what* he felt? I think not. Prof. Brown is both more clear and more candid than Dewey, but no better off.

I here admit all the extreme Behaviourists assert to be the case, *granted their methods* and their *definition of knowledge*. But the conclusion to be drawn from their assertions is not that neither feelings, nor knower, nor introspection exist, but that objective empirical method is not the sole source of knowledge, not the sole route to reality. Dewey thinks he can accept that method, thinks he can get along without either knower or introspection, and yet have both feelings and qualities. To get them, qualities are asserted to be real but ineffable, and feelings are reduced to qualities. The result may be stated as an alternative: Either he does not get feelings, or what he says are feelings are really not

feelings at all. This is only apparently an alternative. There is only one result. No feelings at all. Dewey no doubt has feelings himself. But their reality can never be consistently asserted in his philosophy. The barrier is his blind allegiance to his version of the empirical method. The error he so liberally ascribes to others, he himself commits. He rejects facts to maintain a theory.

If we survey this entire section of Dewey's philosophy we can see that Dewey's rejection of a self capable of immediate knowledge, in conjunction with his exclusive reliance on objective empiricism, accounts for all that is thus far peculiar and erroneous. He cannot have feelings as they actually are, because he has no subject to experience them. The qualities he asserts to be real must be unknown and unknowable because he has no proper subject to know them. And he has no such subject, because by his method he cannot discover one. His very method is restricted in the same way. Empirically I, at any rate, do find I experience both feelings and qualities, and I know both. But this is *unqualified* empiricism. It is only the *restricted* empiricism of physical science which cannot discover such facts as these. That restricted empiricism is justified in science by the nature and aims of science itself; that ever so many leading scientists quite frankly avow the limitation of their method seems to have escaped Dewey. He worships as a metaphysical ultimate what they regard as a practical but limited instrument.

We should none the less admire the consistency with which he develops his position. It is consistency in being not-empirical, but still consistency. And much of what Dewey says about qualities and feelings is true. At any rate I have no disposition to challenge his correlation of the occurrence of feelings with interactions, even with delicate interactions. Feelings do not occur *in vacuo*. I am in agreement with Dewey in his assertion of the objective reality of so-called secondary and tertiary qualities. All the emphasis upon experience being of nature, of things, is sound and timely. Further, both some feelings and some qualities are ultimate. All feelings, especially, are ultimate in that mine are mine and yours are yours, and neither of us can *directly* experience or know the feelings of the other. Simple qualities, too, are ultimate in that they are simple. This simplicity is simplicity of nature and is independent of the complexity of the conditions which govern their occurrence. But this is no barrier to their being known. It is true, as Dewey says, that such qualities can only be denoted, not described. But when thus denoted they are denoted as known. In the words of C. I. Lewis—I quote from a remark he made in discussion—"we can know the inexpressible,"

when "expressible" means extensive description. To this I wish to add that we can and do use this knowledge. Specifically we use this knowledge when we read Dewey and understand what he means when he writes of feelings, qualities, harsh stones, and the rest.

This is my final proof in support of my criticism of this section of Dewey's philosophy. Would it be possible to follow Dewey, possible to have the faintest idea of what he means, if we did not ourselves *know* both feelings and qualities? What would the phrase "harsh stone" mean, if we had "had" such an experience, but never *known* what was experienced? No question is involved of the ultimate ontological status of the stone, or of the correctness of qualifying that ultimate being as "harsh". Similarly there is no question of whether the subject who knows is a Cartesian mind or an embodied self, to use Stout's phrase. The question is simply whether a subject *knowing* his immediate experience is required as the presupposition of the possibility and intelligibility of what Dewey says. This question each of you must answer for himself. In my own case I am certain that only as I have feelings and know them, only as I know the things and their qualities, as found in my immediate experience, is it possible for me to understand what Dewey writes on these matters. I am equally certain that this must be true of Dewey, else he could say nothing. Dewey must use the very type of knowledge he denies in order to make his denial even intelligible. That is the failure of this part of his philosophy. Its falsity is that it is not in accord with the facts.

Genuine empiricism in philosophy is not to be had Dewey fashion, or by any fashion which thinks it has found in some single method the exclusive route to all knowledge. This fatal defect in Naturalistic Empiricism, however, is not without positive import. It gives good evidence that immediate apprehension yields genuine knowledge, and plays an essential rôle in man's general cognitive enterprise. The recognition of this has usually included a tendency to disparage such knowledge as merely subjective, incommunicable, not amenable to critical examination as to its truth or falsehood, and therefore of no value. Here, it seems to me, a careful study of Dewey supplies a wholesome corrective. That immediate cognition requires a conscious subject, is true. It is equally true of every kind of knowledge. But whether *what is known* is properly called subjective or objective, depends not upon the character of the knower, but upon the character of the object known. If that object is a feeling, in the correct, empirical denotation of that term, then no

doubt *direct* knowledge of such an object is subjective, because the feeling, the object known, is subjective. Yet this is not the whole story of this kind of knowledge and Dewey helps us to complete it. "Empirically, *things* are poignant, tragic . . ." etc. (*italics mine*), so begins the by now familiar passage. Who can deny that this is so, that these are aspects of what is called the objective world, that they are known? Dewey agrees as to the objectivity, but holds we merely enjoy or suffer them, directly, immediately, but do not know them. That last is his error. They are known, such has been the thesis of this essay, and in that knowledge immediate cognition plays, I do not say an exclusive, but a dominant part. It would be out of place to attempt here the difficult explication of how immediate and mediate cognition and feelings are intertwined in a single cognitive act. The important thing is that the result is knowledge of nature, objective because what is thus known is objective. The homely remark, how cold the wind feels, expresses knowledge; this knowledge is *inter alia* of the wind; on occasion it is true, it may also be false; and it is frequently communicated, understood. There are more ways of knowing than one, more aspects of reality than any single method can reach. To attach the label, empirical, on any mere one, and then to deny the validity of all the others, merely stultifies the term empirical. It cannot alter the facts.

III.—FINITISM IN MATHEMATICS (II.).

BY ALICE AMBROSE.

LET us now try to see as clearly as possible what precisely the finitist position involves. The formalists have assumed as legitimate those general and existential forms which by hypothesis cannot be demonstrated to be contradictory¹ (those true on grounds of the axioms, or deducible from such as are), and in addition those expressions $(\exists x) \cdot \phi x$, $(\exists x) \cdot \overline{\phi x}$, $(x) \cdot \phi x$ for which either their formal negatives $((\exists x) \cdot \overline{\phi x}$, $(\exists x) \cdot \phi x$, $(x) \cdot \overline{\phi x}$, resp.) or their defined negatives $((x) \cdot \overline{\phi x}$, $(x) \cdot \phi x$, $(\exists x) \cdot \overline{\phi x}$, resp.) have been proved contradictory. They justify the latter on the ground that such an assumption has not run them into contradiction; though this in fact says nothing in this case, since nothing in the symbolism of formalist logic makes sensible the supposition of an alternative to $(x) \cdot \phi x \vee \exists(x) \cdot \overline{\phi x}$. The finitist logic likewise, as Glivenko has shown,² is not such as to admit a third truth-value for any p ; for example, it is impossible to prove the form, "There exists a number which is neither algebraic nor transcendental". Since then it is logically impossible for finitists in this fashion to raise the objection of contradiction, it becomes unclear what objection can be raised and what merit their position has which the formalists' has not. It may well be that in the end the merit it has will be uninteresting in the way matters of taste are, and hence that its being a merit at all will be contested. It may be that a *decision* between the two views ought not to be expected. But it is worth while trying to see what has led finitists to suppose their position has some convincing merit. This means trying to understand more clearly, first, Brouwer's assertion that proofs incapable of evading the assumption that every proposition is true or false³ "lack

¹ R. Wavre, "Logique Formelle et Logique Empiriste", *Rev. de Mét. et de Morale*, Vol. 33, p. 66.

² M. V. Glivenko, "Sur la logique de M. Brouwer", *Acad. Royale de Belgique*, 1928.

³ Let us say, "that every verbal form expresses a truth or a falsity".

mathematical content",¹ and secondly, the finitist demand that expressions should have "concrete meaning".

To that end it is essential to say what it is that the finitist cannot hold with reference to the verbal forms classed as neither true nor false. It cannot be denied, and I believe he would not deny, that such statements as "There exist three consecutive 7's in the development of π " have some meaning. If they did not, one would not know when a verification had been made. Also, though the form as it stands does not suggest a specific and definite activity of verification, it does suggest some activity of a general sort. And we know what it would be like for the above form of infinite range to have a verification, in the sense that we know what it would be like for three 7's to be found in a finite range. That is, "There exist three consecutive 7's in the development of π " is entailed by "There exist three consecutive 7's between the 100th and 1000th places".

It is unfortunate that the language of the finitist suggests misleading objections. The claim that these forms, when unverified, lack "concrete meaning", and that the latter is acquired with the appearance of a verification, provokes the objection, "What possible meaning has 'There exist three 7's in the development of π ' after verification which it did not have before?". And certainly finitists have not made clear in what sense these verbal forms lack sense, how general and existential forms are required to "mean", and what types of verification secure meaning.

Consideration, first, of how these forms differ from forms of finite range will prepare the way for clarifying the answers to these questions which the finitists attempt to give. The difference we are here concerned with is between finite forms and those forms of infinite range which are not known to be true by definition or deducible from forms true by definition. Existential forms of this latter sort we only *may*² be able to verify and we always shall be unable to disprove, if verification and disproof consist in one-by-one exhaustion of instances. Forms of finite range, on the contrary, we are able either to verify or to prove false. A finite number of finite assertions (*i.e.*, assertions involving finite limits) entail any such form, *e.g.*, that three 7's exist in the development of π between the 100th and 1000th places; and one of a finite number proves it. On the other hand, it is hard to give any meaning to the assertion that an

¹ "Intuitionistische Mengenlehre", *Jahresbericht der deutschen Mathematiker-Vereinigung*, Vol. 28 (1919), p. 204.

² The word "may" is intended to suggest dependence on chance.

infinite number of finite assertions entail "There exist three 7's in the development of π ", other than that one can always find another entailing it. That one of an infinite number of assertions would prove it is an assertion exactly on a level with the original assertion, "There are three 7's in π ".

With general forms of infinite range, "verification by exhibition of instances" is meaningless. And verification of "There do *not* exist three 7's in π " is meaningless in the same way. So, given an affirmative general form and an affirmative existential form, in each case one of the two, p or non- p , is extensionally meaningless, that is, an attempt to verify by one-by-one exhaustion of instances is logically impossible.

Now the specification of the sense in which these infinite verbal forms lack sense will evidently concern in some way their verifiability. More precisely, it will appear in the answer to the question, "What would it *be like* for p (or non- p as the case may be) to be *true*?" This is undoubtedly a question about the meaning of p , though still obscure. I wish here to take two typical instances of general and existential forms, and with a view to specifying the senses in which they possess and lack meaning, examine them in the light of this question. First then, the general form, "All numbers of the form $2^{2^n+9} + 1$ are factorable". Do we know *in advance of demonstration* what it would be like for it to be true that any number obtained by the operation of raising to the powers indicated belongs to that class of numbers which have factors? Can we say that we know what it would be like for this to be demonstrated until we have a specific demonstration of it? I think it is clear that we cannot. But if we do not know what is meant by the statement that p is demonstrated, we do not know what is meant by p .¹ That is, the sort of meaning that would be conferred upon it by demonstration is lacking. It might be held that, though no specific demonstration is known, we still know

¹ This is a view which I understood Dr. L. Wittgenstein to put forward in his lectures and which but for them would never have occurred to me. It is only in this sense that any view which I have put forward can be said to have been "guided by suggestions made by him". In stating on p. 188 of my last article that my views were so guided, this was all I meant to say. That is, I did not intend to claim either that I had understood him correctly or that inferences which I drew from what I understood him to mean would follow from his actual views. Any reader who finds mistakes or absurdities in my views must not suppose that he is responsible for them. Even where, as on p. 197 in my last article, I cite an example actually given by him, it must not be assumed that the use which I make of such an example is that which he intended to make.

the possible types of proof which could with ingenuity be made, for example, a direct proof from the construction of the numbers, or a *reductio ad absurdum* proof of "No numbers of this form are prime". But evidently, until the ingenuity to give a specific instance of either of these types is forthcoming, it cannot be said that we know what it is like for the form to be proved true. On the other hand, the form has a sort of minimal meaning, in that we know the definition of factorable numbers, how numbers of the form $2^{2^n+9} + 1$ are constructed, and that it must be proved that such numbers belong to the class of numbers having factors. And we know what it would be like for "All numbers of the form $2^{2^n+9} + 1$ are factorable" to have a confirmation.

Let us examine now the formal negative, "Not all numbers of the form $2^{2^n+9} + 1$ are factorable". We know what it would be like for this to be verified, *i.e.*, for a prime number of this form to be found in some finite range. And thus the negative, non- p , has meaning in this sense. Obviously we do not know what it would be like to disprove non- p , *i.e.*, to prove that all numbers of this form must be factorable.

p itself then has meaning in the sense that we know in general what it would be like to disprove it by the exhibition of a prime number. What is thereby disproved must be merely the general form which means in that minimal sense described above. Disproof by any other type of demonstration, such as proof of "No numbers of the form $2^{2^n+9} + 1$ are factorable", is, in advance of demonstration, meaningless.

Let us turn now to a typical instance of the existential forms in question: "There exist three consecutive 7's in the development of π ". Here we know what it would be like for three 7's to be found within a finite range, in the sense that we know in analogous finite cases what exhibition of a number means; but we do not know at all what it would be like for this form to be true in virtue of any proof other than one dependent on chance discovery. Thus in one sense of meaning, this form p has meaning, and in another sense not. In the case of its formal negative, "There do not exist three 7's in π ", we do not know at all what it would be like for it to be verified. It is logically impossible to run through the entire expansion and thus prove it true, *i.e.*, the phrase "to run through the entire expansion" is self-contradictory. And we do not know what it would be like to prove the contradictoriness of the existence of three 7's. In fact one would be in darkness here as to how to proceed at all, and one has in general no idea of any general proof.

Evidently this non- p is not comparable to the statement that a circle with unequal radii could not exist, since with this latter a procedure for proving contradictoriness is known. In the case of such existential forms then, p itself lacks meaning in the sense that its negative, non- p , lacks meaning, a circumstance not present with finite existential forms. With the latter we at least know what it would be like for them to be false. General forms of infinite range differ from the existential forms here discussed in that of the former we cannot say we are entirely ignorant of what it would be like for non- p to be true. But the two types of forms are alike in that one of the two, p or non- p , is extensionally meaningless.

Because of the view which formalists take of the nature of mathematics, the analysis of these forms here given, in terms of meaning, is for them, in large part, beside the point. But it is best to explain now as clearly as possible what it is that finitists demand in connection with general and existential forms, and then to specify the common ground of the dispute if one exists. Let us first look at general forms. In saying these lack meaning in the sense that we do not know what it would be like for them to be true, it is intended to suggest that they have meaning upon appearance of a demonstration of them. And they have "precisely the meaning which their demonstration confers on them".¹ Demonstration is, as Wittgenstein suggests, essential to the meaning of what is demonstrated. To say a verbal form is the result of a specific proof is the same thing as to say it has a specific meaning. Meaning in the sense in which we know precisely what it is like for a form to be proved true, is what the finitist demands.²

A form having such meaning has, then, with the appearance of proof, a meaning which it did not have before. The reference here to time has laid finitists open to misunderstandings having to do with the difficulties giving rise to theories of "the timelessness of truth". Such theories would treat these verbal forms in question as expressing something true or false, regardless of whether discovery of truth or falsity could be made, and of course regardless of when it was made. Such a sense of truth evidently

¹ R. Wavre, "Y-a-t-il une Crise des Mathématiques?", *Rev. de Mét. et de Morale*, Vol. 31, p. 446.

One of the clearest of such proofs is that in which it follows from the definition of the elements of a given class that they must have some further property.

² Certain restrictions are laid down as to the types of proofs which provide meaning to existential forms.

has nothing to do with verification and hence makes no reference to time. As opposed to this, the finitists use "truth" in such a way that to say "This verbal form expresses a truth" is to say "A method of verifying it *has been* discovered". It should be noted, however, that the proved proposition, like mathematical propositions in general, does *not* make any reference to time. It makes no sense to say "This *proposition* is true now but was not true then"; though, in the case of a verbal form, one can speak of a time when it became true because there was a time when this symbolic form became part of a wider symbolism, the proof.

Reference of another sort is supposedly made to time by the finitist tenet that only those proofs involving a finite number of operations are permissible. The reference is expressed in the consideration that a finite number of operations can be carried out in a finite time. But this in fact says nothing: what would it be like for them to require an infinite time? The special method of proof by enumeration of instances has given rise to such a consideration. An analogous consideration about infinite verbal forms for which no other method of proof is as yet known, has been used to justify the view that such forms always express a truth or a falsity: I mean the view that "just as finite forms can within a finite time and after a finite number of operations be verified or disproved, so may infinite forms within an infinite time and after an infinite number of operations". But the phrase "within an infinite time" is meaningless, and the phrase "after an infinite number of operations" is self-contradictory; so that the claim that a verbal form always expresses either a truth or a falsity is in less difficulty if it ignores entirely any considerations about time.

These verbal forms, however, do have a meaning which is independent of a describable method of verification and which therefore makes no reference to time, or in any case a very indirect one. This is that "minimal" meaning indicated by our knowing what it would be like, e.g., for "All numbers of the form $2^{2^{n+9}} + 1$ are factorable" to have a confirmation and what it would be like for "There exist three 7's in the development of π " to be verified—that is, in the first case, that (e.g.) " $2^{2^{3+9}} + 1$ is factorable" is entailed by "All numbers of the form $2^{2^{n+9}} + 1$ are factorable", and in the second, that "There exist three consecutive 7's in the development of π " is entailed by "There exist three consecutive 7's between the 100th and 1000th places". But knowing in this sense what it is like for a form to have a verification (or confirmation), in

being knowledge only that an entailment holds, by no means involves knowing what it is like for a form to be verified by a given method of proof. The meaning of a form in terms of what it entails or what entails it is quite independent of a specific verification, and hence the truth of the form with only this meaning is likewise independent of verification.

These considerations make it clear that the sort of meaning and the sort of truth finitists require verbal forms to express is bound up with a method of proof, which method does not touch the "minimal" meaning either to confirm or to contradict. That a verbal form has a "timeless" meaning permitting it to be considered in the first place, and that it may *come* to be true (hence that a reference is made to time) are not particularly important. What is important are the stipulations made about the meaning which is acquired with verification. These are in fact stipulations about proof. We shall turn then again to consideration of their requirements.

Let us examine first a requirement, made in connection with proofs of general forms, which constitutes one of the most obscure parts of their doctrine. This is that the functions or classes about which general assertions are made shall have no more than a denumerably infinite number of arguments or members, since otherwise the class or function is not "constructible". This seems to mean that for such general forms as are infinite in range, and which consequently can have infinitely many confirmations, each confirmation must be finite in character.¹ Thus are excluded the "transfinite propositions", such as the axiom that every infinite set can be well-ordered. On what grounds they admit as meaningful forms whose proof is dependent on induction (such as "for every natural number, $m + 1 = 1 + m$ ") is not precisely clear. Obviously such forms are confirmed by propositions of finite nature, *e.g.*, $9 + 1 = 1 + 9$, while not being thereby proved. Certain finitists have denied induction as a defining property of the integers, giving as a reason that every definition requires a supplementary proposition asserting existence,² and that the existence of the series of numbers is neither capable of nor in need of grounding. The series of numbers is postulated as given, and then supposedly the various "constructive" operations are made possible,² and in particular the derivation of the natural numbers by induction. It seems that in the formalization of finitist mathematics

¹ A. Fraenkel, *Zehn Vorlesungen ueber die Grundlegung der Mengenlehre*, (Leipzig, 1927), pp. 51, 52.

² *Op. cit.*, pp. 50-52, for the material of this paragraph.

made in recent years by Heyting,¹ this "postulation of the integers" comes to taking "natural number" as a primitive idea defined implicitly by the axioms. Among these latter is the principle of induction. The constructibility requirement of the finitists can, however, be examined in connection with other instances than that of induction, so that I shall not go into questions about the rôle of axioms or about induction here.

I should like now to examine in the next few paragraphs what constitutes a constructive proof, and how constructibility is connected with the meaning of the general forms we have been considering. It might be claimed that general forms have a meaning which should be sufficiently definite for all reasonable requirements, namely, that derivative from the fact that we know what it is like for their negatives to be true. But do we know this? Let us look at the general form, "All numbers of the form $2^{2^{n+9}} + 1$ are factorable". We know what it is like for it to be true (or false) that a prime number of the form $2^{2^{n+9}} + 1$ exists in a finite range, in the sense that we know in general what the exhibition of a number in a finite range means. And we know what it is like for "There exists a prime number of the form $2^{2^{n+9}} + 1$ " to be true in the sense that it is entailed by an indefinite number of finite forms for which "verification" is understood. In knowing this we know what it is like for "All numbers of the form $2^{2^{n+9}} + 1$ are factorable" to be false. But in some sense, we do not know this: for we do not know *precisely* what it is like for the existential form to be true, but only what it is like for a finite number of the finite existential forms entailing it to be true or false. So in not knowing what it is like for the infinite existential form taken as the negative of "All numbers of the form $2^{2^{n+9}} + 1$ are factorable" to be true, the general form lacks meaning in the sense that we neither know precisely what it would be like for it to be true or to be false. Now we should know what it was like for it to be false were a prime number by chance actually exhibited. The phrase "constructive proof" was not, I think, intended primarily to designate the method of proof which enumerates cases in an infinite series; for such a method, which must rely on chance, cannot be very satisfactory, and can hardly be called a method. But certainly the chance specification of an object which verifies or proves a form false is none the less a specification, and a construction. For the general form to which an exception is

¹ A. Heyting, "Die formalen Regeln der intuitionistischen Mathematik", *Akademie der Wissenschaften zu Berlin, Phys.-Math. Klasse* (1930), pp. 57-71.

exhibited has, with this specification, concrete meaning, since we know in a clear sense what it is like for it to be false. The point I wish to make here is that, contrary to what we at first might suppose, we no more know in advance of demonstration *precisely* what it is like for a general form to be disproved by the exhibition of a contradictory case than by a general proof of self-contradictoriness. With the appearance of either of these disproofs, the general form has concrete meaning, and either proof would count as "constructive". Likewise, a proof of a general form from the properties of its subject class—another instance of a constructive proof—gives that form concrete meaning. We know then precisely what it is like for it to be true. It is important to realize that in order to know what it is like for a form to be true, or false, a proof of truth, or falsity, must be forthcoming. For we cannot know in advance what a mathematical demonstration will be like without actually knowing the demonstration.¹

Let us examine now the existential verbal forms in question, to ascertain what is meant by the requirement that these must have constructive proofs, and what is the connection of such proofs with meaning. For finitists, "to exist" means "to be thinkably constructible".² An existential form has concrete meaning if a specific object or class or function of the sort asserted to exist can be exhibited. Such an exhibition, or "specification", or "construction", is a condition of asserting an existential proposition. In the case of many questions asked in mathematics as to existence, one can see clearly, by looking at the answer given, whether that answer does conform to this condition. And one can say in general what type of proof fails to conform, as well as indicate non-conformity with this condition in specific cases not of this type. I have already made fairly clear I think that the *reductio ad absurdum* method so often used in connection with existential forms of infinite range fails to satisfy the requirement to specify what is claimed to exist. To recapitulate: The schema of this method of proof is as follows: "Given the disjunction ' p or not- p ' (e.g., 'For all $n > 2$, $x^n + y^n \neq z^n$, or there exists an x, y, z , and an $n > 2$ for which $x^n + y^n = z^n$ '). Suppose p is proved false. Then not- p is true." Here is a proof which either employs the law of excluded mean regardless of the nature of the mathematical forms related (formalist procedure), or employs it to conclude to existence precisely on the ground that the two forms are proper values in an exhaustive disjunction (logistic procedure). Finitists deny that they are proper values,

¹ Cf. p. 319, note.

² A. Fraenkel, *op. cit.*, p. 38.

and hence that the law of excluded mean is *applicable* to these formal contradictories (though this is not to deny the law of excluded mean). For this reason they exclude, as a non-constructive method, the inference of existential forms from proof of the contradictoriness of the forms taken as their negatives. A proof of this sort, which proves the non-contradictoriness of the existential form, does not thereby demonstrate the existence of the class or argument denoted by the subject concept. A "formal" method of proof is permitted in concluding from a constructively demonstrated form to the falsity of its negative, but not in concluding from a contradictory form to its existential negative, "without the contradiction mediating a way to the construction of the concept" ¹ denoting the class said to exist.

In discussion of constructive proofs of general forms, and of the sense in which certain of the questionable general forms lack meaning, I held that the chance discovery of an object which proved the general form false was a proof quite as acceptable as any other, once carried out (though not in general satisfactory as a method), in that such a disproof of the general form (or proof of the existential form taken as its negative) provided the form with a meaning which it did not have before. I should like to examine this claim a little more closely with respect to such existential forms as "There exist three 7's in the development of π ", in an attempt to say how proof by a chance discovery of a verifying object conforms to the requirement of meaning which other "constructive" proofs satisfy. The *reductio ad absurdum* method of proof has been shown to fail to conform to this requirement; and for a different reason, proof by a chance discovery might also be considered unsatisfactory. It might be thought that a chance verification in a series of trials would provide a form with no meaning which it did not already have, just as verification of a finite form in the course of a series of trials provides it with no meaning which it does not already have. The distinction between the two lies in the fact that with the one we know precisely what verification would be like, while with the other we do not. In the case of both finite and infinite forms we do know what a verification would be like in the sense that we should know in advance, where our method is enumeration of cases, the kind of thing we should call a verification. In the case of infinite forms we do not know what the specific verification would be like of which we should say, "At this step of the enumeration the verification is made"; but we need not know this in the case of finite forms. The sense in which we know

¹ A. Fraenkel, *op. cit.*, p. 36.

precisely what it is like for a finite form to be verified and in which we do not know this for an infinite form, is that we know in advance what it is like for a verification *to be carried out*. Actually carrying out the verification between the limits 0 and 6 gives the finite existential form no meaning which it had not already, for one already knew what it was like for verification to be effected in this range or in a smaller range. On the other hand, we do not know at all what it is like to carry out a verification, say, by use of any of the five arithmetical operations, or expansion in series, or integration, or trials throughout a series of integers, between the limits 0 and ∞ . ∞ is no proper limit here, for it is no number. Description of the verification of infinite forms in terms of these "limits" is intended only to call attention to the fact that the range in which it could be proved is completely indeterminate and that there is no *range* in which it could be disproved. In contrast, the range in which such a form as "There is an even number between 5 and 10" could be either *proved or disproved* is determinate. The point I wish to make is that knowing precisely what it is like for an infinite form to be verified consists in knowing the specific verification, while we know precisely what it is like for a finite form to be verified in the sense that we know the range in which verification or disproof can be carried out. The one cannot be known in advance of actual demonstration, the other can. Hence it is that demonstration in the one case provides a form with a meaning it did not have before, and in the other case not. And for this reason, verification of an infinite form by the chance discovery of a verifying object is acceptable as a constructive proof.

Inasmuch as the phrase "constructive proof" was very likely not intended primarily to designate proofs made by chance, I should like to indicate what further could be meant by the phrase. Any proof of a general form from the properties of its subject class, or any verification of an existential form in which operations of some sort function in specifying the object asserted to exist, are of the sort required. If, for example, some method were known for constructing three consecutive 7's in π , we should have an instance of a "thinkable construction". It is important to see that a demonstration showing that an object with the property in question must exist is not sufficient; of such a demonstration it is further required that the object be specified. An example of the type of proof demanded would be that demonstrating the existence of a limit to a sequence *and* specifying what the limit is.¹ Another illustration is that given

¹ If, as Weyl claims, Cauchy's convergence principle is left intact by finitist requirements. See *Das Kontinuum* (Leipzig, 1918), pp. 57-61.

by Fraenkel in connection with the fundamental theorem of algebra : a proof that an equation of n th degree has n roots in such a way that "the proof method makes possible the numerical reckoning of the roots of a given equation with all desired precision".¹ What such a proof would be like we cannot know in advance, but, once produced, it seems in accordance with usage to claim that the fundamental theorem has acquired a new meaning.

In this connection I should mention a misconception about the phrases "possibility of finding", "*thinkable* construction". Such phrases have, I think, been used to prevent the interpretation that an *actual* construction is necessary when an assured method of construction is at hand. In this latter case an actual construction would be no more required than an actual process of multiplication in a case where it was known that such an operation would provide the answer to the question asked. So the claim by some finitists that what is required for concrete meaning is in all cases an actual demonstration is simply wrong.

We have seen now that, throughout, the finitist analysis of general and existential forms has been carried on with some reference to meaning. Formalists have declared that all properly mathematical assertions are meaningless combinations of signs. The question then arises, What possible common ground can there be for dispute between the two schools? Are they disputing at odds? It would appear that all the finitist claims could be brushed aside by the observation that such claims were of no interest to the formalist school, and that meaningfulness was not a virtue of a verbal form, but an irrelevance. It may well be that the two disputants will eventually reach a point of irreconcilable difference, and that this point concerns their views on the nature of mathematical propositions and of mathematical proof. Evidently it is their differing conceptions of mathematics, as Becker notes,² which determines their respective prime concerns : for formalists the non-contradictoriness of axiom systems, for finitists, the problem whether a given alternative is decidable, a given problem solvable (by a constructive method). One cannot really say of the formalists that one of these problems is their prime concern, since for them the problem of finding a rule for deciding whether a given formula is derivable comes to asking whether this formula leads to contradiction. The point is that it is not their prime concern to find a rule for deciding

¹ *Zehn Vorlesungen ueber die Grundlegung der Mengenlehre*, p. 49.

² "Mathematische Existenz", *Jahrbuch für Philosophie und phänomenologische Forschung*, Vol. 8 (1927), pp. 624-625.

whether a given formula is derivable by certain "constructive" proof-procedures. This is the difference between them.

It might appear as a point in favour of the finitist position that, starting from axioms assumed to be non-contradictory, the non-contradictoriness of any specific form is secured automatically once one has obtained according to logical rules the existence of an object by construction; whereas the converse is not always the case. But this will not be a merit if one is disposed to prefer for mathematics the less stringent condition, namely, formal non-contradictoriness. What then remains as common ground for dispute? Perhaps this question can be answered by examining the points where they disagree as *mathematicians*. Even the finitists have discussed meaning only when called upon to defend the criticisms they have made of formalist methods of concluding to existential forms. So it would appear that though formalists and finitists cannot dispute over meaning, they can dispute over types of proof. *Reductio ad absurdum* proofs are at the centre of the dispute, since such proofs are in certain cases and by one school excluded as illegitimate, while constructive proofs would be accepted as legitimate by both schools. Should the formalists admit that these latter proofs were for any reason preferable to the others, their case would I think be prejudiced. For surely their reason must then be meta-mathematical, and it could not merely be that such a proof had the virtue of being non-contradictory, since the other proofs also presumably have this character. It might be, however, that they could justify their choice without any reference to meaning; or else, possibly, that they never would prefer a constructive proof to a non-constructive one—in either of which cases finitists would have gained nothing and the dispute would still lack any common ground on which settlement might be effected. I cannot help feeling that if a formalist mathematician were confronted with the two sorts of proof for an existential form, he would choose the constructive one. But this is conjecture, and inconclusive.

A final possible point of contact between the two schools concerns the sort of statements formalists call "meta-mathematical". Hilbert's axiom of the solvability of every problem appears to be of this nature. The forms at issue in this controversy have been treated as if the assertion of solvability were pertinent to them, that is, as if they were mathematical. I should like to mention a peculiar feature of some of them which throws some doubt on this. Certain forms at issue, which have non-constructive proofs, and other "results" of proofs as well, play a very peculiar rôle in the actual proofs.

Instead of appearing as a conclusion of the formal proof, they are like a non-formal commentary upon the proof, having no essential place in it. "Every equation of n th degree has n roots" and "There exists a derivative of the function $f(x)$ " are instances of "comments" on proofs. In the latter case one of the things which can occur as the conclusion of the proof is a symbolic form identical with the definition of a derivative. "There exists a derivative of the function", or even in mathematical symbols, " $\exists dx \dots$ " seems then in a way superfluous. "There are exactly n roots to any equation of n th degree" is likewise not a part of the strictly formal proof. And in many cases the forms at issue are such as could not, I think, be the formal conclusion of a formal proof.

It has been suggested¹ that these are devices which "catalogue" proofs. Could it be that from the formalist viewpoint these forms must be relegated to a position much more akin to what they have called meta-mathematical statements than strictly mathematical ones? What finitists have said about the nature of these forms, and what they require of the proofs which these forms catalogue, remains unaffected by an affirmative answer. But if formalists would agree to this suggestion about these forms, as I believe they might, then what they have said about the nature of them will be altered; instead of being treated as strictly mathematical forms—as merely contradictory or non-contradictory, they will be treated as having meaning. But their meaning is unlike that of meta-mathematical statements which assert, for example, that a meaningless row of symbols is deducible from axioms. In cataloguing the formal processes, a form serves as a commentary, as an interpretation of those processes. It is meaningful then in a different sense than a statement about the symbolism of the proof such as, "This proof is non-contradictory"; it is meaningful in the sense that an interpretation is. And the commentary is interpretation of the formal deduction; that is, the formal deduction itself is taken as meaningful. On the other hand, what the commentary comes to, what it means, is to be found by looking at the symbolism of which it is taken as an interpretation.

These it seems to me are conclusions formalists might come to about statements at the end of such proofs as those for the fundamental theorem of algebra, and of such other proofs as have been questioned by finitists.² But they are not forced to

¹ By a member of Dr. Wittgenstein's discussion classes.

² Evidently here we are not considering questionable forms for which at present no method of proof whatever is known, as it would scarcely be sensible to call a form a commentary which did not comment on proofs possible or already made.

these conclusions, as they might well deny that such commentaries need be made, that such interpretations were called for. This they could consistently do. Only if they continued to consider as mathematics those traditional portions of which these commentaries are a part, and agreed to the description of these commentaries just given, would they and the finitists have a common point of disagreement. In this event, finitists would disagree that a commentary such as "There are n roots to an equation of n th degree" has the meaning which the use of the word "exists" suggests it has. That is, the dispute would be one about meaning. If formalists granted this, but were willing to use the word "exists" with all these meanings, I do not know that any further considerations would clear up the dispute. The word can be used as each pleases. In cases, however, where inference is made from $(x) \cdot \phi x$ to $(\exists x) \cdot \phi x$, there does seem to be no point in the inference if $(\exists x) \cdot \phi x$ means only that $(x) \cdot \phi x$ is self-contradictory. If one proceeds from the tenet that the conclusion of a proof (or a commentary on a proof) means what the proof shows it to mean, it would seem that a proof of the equivalence of $(x) \cdot \phi x$ and $(\exists x) \cdot \phi x$ would be necessary in the case of infinite forms. $(x) \cdot \phi x$ has been defined by Russell as $(\exists x) \cdot \phi x$, and this is legitimate in the finite case where proof of $(x) \cdot \phi x$ is at the same time proof of $(\exists x) \cdot \phi x$ (where the latter means that an object for which ϕx is false is specified). But in cases where the proof of $(x) \cdot \phi x$ is not the same as specification of an instance contradicting $(x) \cdot \phi x$, what grounds has one for making the two symbols equivalent in meaning? This seems to be no objection to the view which does not accept the tenet that proof is essential to the meaning of the conclusion, and which in fact takes the symbols of the conclusion, and hence the conclusion, to be meaningless. And it is not clear to me whether one can ask for a justification in the symbolism for making the *usage* of these symbols interchangeable. It would seem that the procedure of equating the different usages of $(x) \cdot \phi x$ and $(\exists x) \cdot \phi x$ within mathematics and that of defining these symbols in terms of each other by means of the transfinite symbol $A(\epsilon_x A(x))$ (which ignores differences of usage), require justification. But it is not clear what such justification would be in terms of anything which referred to meaning as finitists conceive it.

Before turning away from this subject I wish to make one further comment on this whole discussion of meaning, in particular, on the phrase "concrete meaning" which I have used.

This latter suggests, and has been so used as to suggest, (1) that one can talk of a verbal form coming to acquire meaning as though meaning was such as to exist first apart from any symbolism, and (2) that in examining forms for concrete meaning one knows apart from any proof what meaning the existential sign has and that certain proofs provide conclusions whose meaning is this. The misleadingness of the first suggestion of the phrase: "A verbal form can come to acquire a meaning", is evident in the question it tempts one to ask: "What is it for a form to acquire meaning?" Finitists have replied, "It either has proof or proof is possible". Accepting this reply, however, one still wants to ask further, "What happens when the form has a proof—how can proof give it meaning?" The question seems to be asked because of the assumed necessity for some relationship to be established between meaning and symbolism; and one never sees how this is secured.

This suggestion of my language, that symbolism is in no way essential to meaning, Brouwer has even gone so far as to assert, along with other errors connected with it: symbolism, he has held, is an auxiliary to mathematical meaning, and an inessential one.¹ Brouwer has, I think, expressed his point of view in saying that language is merely the convention by which a mathematical fact is represented, and the fact itself is independent of this convention.² The language, say, of algebra, functions as an instrument of communication, and its merits and demerits are those of a translation. Individual symbols are of course on this view in the same case with the symbolism for a mathematical proposition. For example, in the development of the natural number series one need not use either the symbols for particular numbers or the operator symbol $+1$. Again, it is said that the language of logistic not only is inadequate to formulating the reasoning in mathematics in the way a finite number of rules is inadequate to the possibilities of thought;³ it oversteps the limits within which it is a translation: $p \vee \sim p$ ceases to be a translation of the reasoning about finite mathematical facts and sets itself up as a rule to which all mathematical reasoning must conform.

Here we have in its crudest form the supposition that meaning and any symbolism whatever which may express it can be iso-

¹ "Intuitionism and Formalism", *Bull. of the Amer. Math. Society*, Vol. 20, p. 86.

² P. Brouwer, *L'idéal scientifique des mathématiciens* (Paris, 1920), p. 170.

³ A. Heyting, "Die formalen Regeln der intuitionistischen Logik", *Akademie der Wissenschaften zu Berlin, Phys.-Math. Klasse*, 1930, p. 42.

lated from each other and discussed as though their connection was merely that symbolism is useful for communication. This is to say much more than that the presence of symbolism is no guarantee of the presence of meaning. The latter is in fact the case, but not because symbolism in general is an accompaniment of meaning and in no way essential to it. This idea of the disjunction of meaning and symbolism, however, has expressed itself not only in the view of certain finitists here described, but also in the claim of logicians that an existential form has meaning, asserts the existence of a particular mathematical fact, although one has no way of expressing this fact (no symbolism for it). And because my use of the phrase "acquisition of meaning by a verbal form" might suggest that meaning is not necessarily bound up with symbolism (though it remains true that a particular symbolism may not be meaningful), I have introduced the preceding discussion. I wished also thereby to make clear that my insistence that a proof is essential to the meaning of its conclusion involves the view that symbolism is essential both to proof and conclusion—a view which cannot be ascribed to Brouwer or such finitists as hold that symbolism is inessential to either. I shall not criticize their view in detail; I think one can see the various objections to it. For one thing, one need only try to think of the meaning of a sentence apart from some symbolism.

The assertion that a certain form has meaning or that it is meaningless in the finitist sense does not then refer to a "content" which a particular symbolism may or may not convey. I wish to say that it refers to the symbolic system of which the form in question is a part. To say that a form has meaning is to say that in the symbolic system an answer to the question whether it is true, or false, is provided for.¹ If a question is asked for which no answer is provided in the system (that is, for which no answer *can* be given), then this question is meaningless. And if the form about which the question is asked "acquires meaning", this is not some peculiar acquisition of "content"; rather, the form has become an integral part of a new system, it has become the conclusion of a proof which is a new bit of mathematics. And this proof, on which the meaning of the form depends, provides the answer to the question whether the form is true or false.

The second suggestion of the usage of the phrase "concrete meaning", that in examining forms for concrete meaning one

¹ This, or a view similar to it, has been put forward by Dr. L. Wittgenstein. Cf. p. 319, note.

knows first, apart from any proof, what meaning the existential sign has, and then finds that certain proofs give conclusions in which the existential sign has this meaning—this suggestion is bound up with the suggestion that in any conclusion which has concrete meaning the meaning of the existential sign is the same throughout. It is continued discussion of existential forms in general which produces this latter illusion, and the illusion that one knows already what "existence" means and can investigate whether the meaning in each particular occurrence of the word is this.

Now one does in fact know in a sense what "existence" means. Finitists have laid down the general definition of signs of the form $\exists x. (\phi x)$ as "An object x can be specified for which ϕx holds".¹ This is really a requirement that signs of this form be used only when an object can be specified. The meaning given by this definition to the form $\exists x. (\phi x)$ serves then as a standard by which all expressions of this form are tested. But I wish to say that in spite of this, a particular expression of this form does not have the same meaning when it occurs as the conclusion of a proof as when it does not. I have insisted that to say an existential form has meaning is to say that it has proof (or that proof is possible) and that the form gets its meaning from the *particular* proof it has. It is then seen to be nonsense to hold that in all cases of existential proofs whose conclusions have concrete meaning, the existential sign in the conclusion has only the meaning stated by the general definition. For this is like saying that a symbolic expression, the meaning of which is bound up with its proof, is identical in meaning with the same expression when its meaning is arbitrarily laid down by definition. The existential sign here has a meaning which conforms to the requirement expressed in the definition, but one which is not identical with that which the definition assigns to it.

If now there is no one meaning for the existential sign in conclusions of proofs, what exactly is being said when certain conclusions are claimed to have proper meaning and others not? I wish to hold, for the finitists, that something is being said about the symbolism (proof) from which they follow. The criticism of the traditional use of the existential sign in the conclusions of *reductio ad absurdum* proofs and certain others, is in part that

¹ See A. Heyting, "Die formalen Regeln der intuitionistischen Mathematik", *Akademie der Wissenschaften zu Berlin, Phys.-Math. Klasse*, 1930, p. 58. This is in fact an explanation, and obviously not a formal definition. $\exists x. (\phi x)$ is undefined in the calculus.

the proofs are not other than they are—that they are not proofs which exhibit the object or function said to exist, and in part that the phrase “there exists” appearing in the conclusions suggests that they are such proofs. The requirement, then, of “concrete meaning” amounts to a requirement of a certain type of proof, of a certain type of answer to questions in mathematics. This really comes to saying that the preference of certain proofs to others is not justified by saying, as I have previously done, that certain proofs yield conclusions with a more satisfactory meaning. This does not justify the preference, but states it over again.

The dispute between formalists and finitists over what proofs are acceptable in mathematics is then, of course, unaffected by the remark that preference of certain proofs to others is not justified by their claim to more satisfactory meaning for their conclusions. This remark is not evidence for either side, for it states merely that to say a certain proof is preferable to another is to say its conclusion has a more satisfactory meaning; the latter can be no justification for holding it to be preferable. Actually then I have merely stated what follows from the view I have put forward for the finitists, that to say a form has meaning is to say it has proof. Nothing has been said which affects the dispute about what types of proofs are admissible.

Within finitist mathematics preferences among proofs would of course be non-existent because unsatisfactory proofs would be excluded. Once having arbitrarily laid down a requirement, say, for the use of the existential sign, it is only significant to hold a proof conforming to this requirement to be preferable to another which does not, if one has a system in mind, *e.g.*, the mathematics of logicians, where both uses of the existential sign occur. Here the preference would be justified by pointing to the requirement laid down; a justification beyond this will lie outside either system, having to do perhaps with certain satisfactions in the use of symbols. Differences in such matters may be the source of the dispute between the various schools of mathematicians, and such differences will perhaps never be eradicated. What proofs shall be admitted into mathematics, and the related questions, whether meaningfulness or non-contradictoriness shall be required for mathematical forms, and whether mathematical forms are to be described as meaningful or meaningless—these questions will all perhaps remain undecided.

I wish now to examine briefly the actual consequences of the finitist position in analysis and other branches of mathematics. The restrictions on generality have by some been signalled as

the knell of analysis. Weyl says that any general theory of functions or of sets is no longer tenable. It is disturbing, if "to say that anything possibly involving an infinity of any kind must be meaningless is to declare in advance that any real theory of aggregates is impossible".¹ I cannot give here very much of the constructive work of the finitists in analysis and the theory of powers, and I shall try to give only enough to provide a basis for a treatment of their work of destruction. First, it is to be observed that difficulties about general and existential forms involving denumerably infinite sets of natural numbers do not preclude the introduction of these sets when one comes to construct a theory of powers—or at least those for which a "constructive" definition can be given. And that a class shall have such a definition is expressly stipulated. So far as I can understand, this means that classes must be given by a law which generates from every number a next, as, for example, the series of integers is generated. Or they must be given by a law of correspondence. An example of this is $m_k = n_1 + n_2 + \dots n_k$. The elements of the set m_k correspond to each series $n_1 + n_2 + \dots n_k$ and to each order number k of the series.² A simple case of this correspondence is that holding between two integers, $N = 2k$, which defines the set of even numbers. Construction of sets in the narrow sense means one-by-one exhibition—a process evidently applicable only to finite sets. Classes not constructible in this sense or by a law of generation, for example, the set whose elements are the points of space (which formalists nevertheless consider legitimate), finitists reject.

The above-described definition of classes is to be contrasted with the type no longer generally admissible. Russell said, "Classes which are infinite are given all at once by a defining property of their numbers, so that there is no question of 'completion' or of 'successive synthesis'".³ This construction of a class by means of a property of the elements, postulated as legitimate by Zermelo's axiom of inclusion, is considered by finitists as the root of some of the paradoxes, like the ordinal number of all ordinals. It is what Weyl calls the "objectification"⁴ of properties, illustrated in this paradox, and in the

¹ F. P. Ramsey, *Foundations of Mathematics*, pp. 74-75.

² R. Wavre, "Y-a-t-il une Crise des Mathématiques?" *Rev. de Mét. et de Morale*, Vol. 31, pp. 457-458.

³ *Our Knowledge of the External World as a Field for Scientific Method in Philosophy* (1915), p. 156.

⁴ "Consistency in Mathematics", p. 249. See also discussion, "Ueber die neue Grundlagen-Krise der Mathematik", *Math. Zeit.*, Vol. 10, p. 44.

definition of a real number as a class of rationals with the section property (as a property of properties), which makes the theory of types necessary. And the suspected axiom of reducibility has only been formulated owing to the supposed need, in certain cases, of "levelling" types. It is possible that here one error generates another to remedy it. At any rate Weyl rejects the axiom of reducibility and objects to the treatment of properties and relations as arguments between which new relations can hold, that is, to their "objectification". And the definition of classes by a characteristic attribute, the first step in the direction of objectification, is subjected to severe criticism. However, inasmuch as finitists claim that they differ from the formalists only in method but not in results where finite classes are concerned, I should think the injunction against property-definitions would not be extended to finite classes—except in so far as these tempt one to treat the infinite case analogously. Only with infinite classes are difficulties at all obvious.

The sort of difficulty presented by infinite classes defined by properties is as follows: Consider the statement, "Every infinite set contains a denumerably infinite sub-set". If a denumerable set is defined as one which can be set in 1-1 correspondence with the series of natural numbers, and if a set is defined generally as a law of generation or of correspondence, one can see that it might not always be possible to specify such a law in every infinite set, where the term "infinite set" includes sets not possessing the cardinal number of the set of integers as well as those which do. Again, consider the statement, "In every aggregate of integers there is a smallest one". Suppose an infinite class were given by a property which did not order the elements of the aggregate. How would one in that case be sure that a first element existed, when the only method of determining it would be to run through the aggregate? A difficulty is also involved in the formalist claim that every class can be well-ordered. This will be seen by examining the multiplicative axiom to which it is equivalent: Given a class k with an infinite number of sub-classes, there exists a class consisting of one term from each sub-class of k . The difficulty here is in finding a rule of selection. Russell points out that this axiom is also equivalent to the claim that of two unequal cardinal numbers one must be greater than the other.¹ Difficulties only arise with this claim when we come to transfinite arithmetic: \aleph_1 , the cardinal number of the set of all denumerably infinite

¹ *Introduction to Mathematical Philosophy*, p. 124.

ordinals, and 2^{\aleph_0} , the number which Cantor assigned to the class of real numbers between 0 and 1, are a pair for which it is doubtful whether one is greater than the other. But first of all, because of the limitation of classes to those for which we can give a constructive definition, finitists refuse even to admit the existence of classes whose powers are not \aleph_0 or less. \aleph_0 is the only infinite power whose existence they recognize. This means that in analysis we are no longer to consider such a set as "the set of numbers between 0 and 1", "the set whose elements are the points of space".

We have been accustomed to defining the continuum as the aggregate of all real numbers, which, according to the finitists, is just as illegitimate a totality as, for example, the set of all denumerably infinite ordinals. We have defined its elements, the real numbers, as classes of rationals constructed by the infinite process of dividing all the rationals into two sections. Such a process can obviously not be carried out, and for this reason the basic principle of Dedekindian section is denied by the finitists any legitimate place in analysis. Theorems which use it are unacceptable; for example, the theorem proving the existence of a least upper bound to an aggregate of real numbers. The question then arises as to what construction can be given of real numbers; for since finitists wish the elements satisfying any axioms to be constructible, *i.e.*, since it is not sufficient that an axiom system satisfied by real numbers be given, some construction of real numbers must be specified if finitist mathematics is not to omit considerable sections of traditional mathematics. The reply is that a real number is a law of generation—of the decimal expansion, for example. The process of extracting the square root is such a law. A law of this kind gives a real number as a decimal whose approximation may be pushed as far as desired. Now any real number known up to the n th decimal will lie in the interval bounded by two rational numbers $\frac{i}{10^n}$ and $\frac{i+1}{10^n}$ (i is the number formed by the n figures of the development, *e.g.*, in .666 . . . , $i = 666$, $n = 3$). As we shall never have more than a finite number of figures of the development before us, the number will be represented by the sequence of intervals whose limit we shall never reach. Brouwer has defined the real number independently of the decimal expansion as a sequence of intervals, each enclosing the next, with their length approaching zero as one progresses in the sequence. The ultimate elements of the continuum are not points, then,

in any case, but intervals which are themselves each a new continuum.¹

Is now the continuum a law of generation of the laws defining real numbers? The answer is "No", since such a law would determine the choices of the decimal expansions. Prof. Wavre,² giving in essentials Weyl's conception of the continuum, interprets the continuum as consisting of series of arbitrary choices, where no law limits the freedom of choice nor determines after n choices what the next shall be. Evidently the numbers are not all given (the continuum is not constructible out of such series), and the definition of the continuum as the aggregate of all real numbers is precluded.

How much of mathematics may suffer through the restricted definition of classes, the cautious use of the words "all" and "there exists", the new conception of the continuum, and the rejection of the multiplicative axiom, the axiom of reducibility, and the principle of Dedekindian section cannot very well be estimated. Lebesgue and Borel have already replaced demonstrations calling for suspected principles by others not invoking them, and it is possible that the labour of making acceptable some of the unacceptable theorems will be trivial. Division among the finitists themselves concerning what statements could be given sense has naturally heightened the protest against them. Weyl's treatment of "All numbers are even"³ reflects doubt on such a surprising claim as: It is groundless to assert that 1 is the smallest cardinal number following 0. And where a paradoxical denial such as "a cardinal number is neither $= 0$ nor ≥ 1 " can be dealt with fairly simply, it seems obvious it should be. If we have an infinite lawless aggregate of numbers from which we construct a class of those numbers, say, with the property "even", we can see that the cardinal number of this class will be indeterminate. But the extremely general account of this denial about cardinals⁴ was far from suggesting such a simple interpretation. However, the loss to mathematics, the specific errors, and the unnecessary paradox are no conclusive condemnation of the examination of the legitimacy of infinite processes and of the demand for constructive proofs for the verbal forms to which one concludes. I once thought that the decision between the stipulations of mere formal non-contradictoriness and of concrete meaning laid down by the formalists and finitists

¹ See "Y-a-t-il une Crise des Mathématiques?", pp. 452-453, for discussion of the definition of a real number.

² *Op. cit.*, pp. 452-455.

³ See my former article, pp. 201-2.

⁴ H. Weyl, *Ueber die neue Grundlagen-Krise der Mathematik*, p. 67.

for mathematical statements, would rest on which proved finally to be the best methodological maxim. This is probably true in fact, but the finitist position has, I think, much more to recommend its adoption than the possible decision which a rather slowly changing practice may award it. At any rate, it hardly characterizes the view to call it, with Ramsey, "the Bolshevik menace of Brouwer and Weyl".¹ Ramsey changed his views of the finitists' claims, but the objections to them still take a form suspiciously like the outcry of vested authority.

¹ *Foundations of Mathematics*, p. 56.

IV. DISCUSSIONS.

A DEFINITION OF ABSTRACT SYSTEMS.¹

THIS paper is concerned only with abstract systems. That is to say, it is concerned with systems in which the postulates and theorems are schematic, not constituting propositions, but being merely the forms or, if one will, the skeletons of propositions. A simple example of such an abstraction is found in the postulates for serial order, the postulates of irreflexiveness, connexity and transitivity.² In these postulates there is no explanation of what actually is the relation involved, or what kind of elements it relates; indeed, it is possible to point out many concrete interpretations of these postulates, dealing with a variety of relations and diverse groups of elements. The collection of positive integers, ordered by the relation "is greater than," and any group of lineal descendants ordered by the relation "is an ancestor of," are familiar examples of such systems satisfying the postulates of serial order. But the postulates themselves are not concerned with either of these relations, they have to do with the form common to them.

One of the most important problems arising in connection with abstract systems, is the question of proving consistency. When one has assembled a group of abstract postulates, how shall he be sure that it does not contain implicit contradictions such that contradictory theorems may be deduced? From an æsthetic point of view, perhaps, a system which leads to contradictions may be just as satisfactory as one which is consistent, but it is generally held that reality must be consistent, so that an inconsistent abstract system can never be exemplified in the real world and is forever doomed to remain a bare skeleton. From a utilitarian standpoint, therefore, consistent systems are to be preferred.

This feeling that reality must be consistent, has given rise to one of the most commonly discussed modes of proving consistency—that of finding examples of concrete individuals and relations which exemplify the postulates.³ The argument underlying this method of proving consistency is as follows: Since reality is consistent and since a part of it has the form described in the abstract system, that form itself must be consistent. Thus the abstract postulates for serial order may be shown to be consistent by considering a group of three

¹ Presented before The Eastern Division of the American Philosophical Association on 27th December, 1934, and revised.

² Cf. E. V. Huntington, *The Continuum*, p. 10.

³ Cf. R. M. Eaton, *General Logic*, p. 474 n.; Lewis and Langford, *Symbolic Logic*, p. 346 ff.

instants of time, say nine, ten and eleven o'clock this morning, together with the relation, "is earlier than". Here nine o'clock is earlier than ten o'clock, nine o'clock is earlier than eleven, and ten is earlier than eleven—in these cases the relation holds, in all others it fails. All the postulates are satisfied by this example, and the consistency of the postulates may be proved in this manner.

This example is a sufficient proof of consistency, but it is not necessary to the proof. Any three times would have done just as well, as would some examples which have nothing to do with time, say a dollar bill, a two dollar bill and a five dollar bill, considered in connection with the relation "is more valuable than". Here the five dollar bill is more valuable than the two, the five dollar bill is more valuable than the one, and the two dollar bill is more valuable than the one. In all other cases the relation fails. Similarly many other examples might be used.

When we come to inquire as to the common character of all these examples, we find striking diversity in the elements and relations with which they are concerned, but identity as to form. In the case of every example of three elements used to satisfy the postulates for serial order, the relation—whatever it may be—holds between one element and each of the others, taken in that order and, among the latter two elements, the relation holds between one and the other. In all other cases the relation fails. This, then, is the pattern or schema of all the examples of three elements satisfying the postulates.

When we consider this pattern, or schema, for its own sake, however, what we are dealing with is not any concrete example, but an abstract example, in just the same sense in which postulates may be abstract. It is a form of examples, capable of being considered by itself, apart from any consideration as to whether or not it is actually exemplified in the real world.

When we come to examine actual proofs of consistency as they are most often given in postulate theory, we find, moreover, that they employ not concrete examples, but abstract examples.¹ Consistency is assumed to have been proved not by reference to any group of actually existing entities and relations, but by means of an abstract counterpart of these. Thus the principle of proving consistency by means of exemplification is given up, and the problems arise: What are the relations between these two sorts of abstractions—abstract postulates and abstract examples? and, second: If the consistency of postulate sets requires proof, is not similar proof required in the case of examples?

The second question, as to the consistency of abstract examples, is easily answered. From their very form it is apparent that examples must be consistent. The abstract example, we have seen, specifies that the relation holds in the case of certain permutations of elements

¹ See, for example, E. V. Huntington, "A new set of postulates for betweenness with proof of complete independence," *Trans. Amer. Math. Soc.*, vol. 26 (1934), pp. 257-282.

and fails in others. Thus the schematic example of those elements satisfying the postulates for serial order may most easily be stated by assigning proper names, say "1," "2," and "3," to the elements. The example may then be formulated in the following terms: The relation holds in the case of the ordered couples 12, 13 and 23. It fails for the couples 11, 21, 22, 31, 32, 33. Since there is only one statement about each permutation of elements no chance for inconsistency arises; and an example is necessarily consistent, just as is a group of singular propositions provided no two of them have the same subject matter. There can be no question, then, as to the consistency of abstract examples.

This account of an abstract example as dealing with a group of elements, with the holding or failing specified for each permutation, opens up interesting possibilities. If we know the number of elements involved and the degree of the relation, we can determine all the examples which can possibly be constructed. Thus, given a single element in the example and a dyadic relation, there can be two and only two examples, the one in which the relation holds and the one in which the relation fails. With two elements and a dyadic relation there are four permutations of elements taken two at a time; and there will be sixteen possible examples, varying from the case in which the relation holds for each permutation, to the case in which the relation fails for each permutation. These are all the possible examples for two elements and a single dyadic relation. When dealing with cardinalities of two or greater, some of the examples will be found to have identical properties; but these can be determined in terms of the nature of the permutations of elements, and duplicate examples may be eliminated from the list.¹

In a similar manner, all the examples consisting of three, four, and n elements and a single dyadic relation, may be conceived, and duplicates eliminated. Similarly, this procedure may be extended to transfinite cases and we may imagine set out before us all the examples, of whatever cardinality, involving a single dyadic relation. We may refer to this aggregate as the *field* of a single dyadic relation. In an analogous fashion, we might speak of fields of triadic, tetradic, one dyadic and one triadic, two dyadic relations, etc.

¹ To give an instance, the example of three elements in which the relation holds for the couples 13, 21 and 23 and fails for the couples 11, 12, 22, 31, 32, 33 has identical properties with the example quoted above. The only difference between the two is that the element named "1" in the first example has been named "2" in the second and *vice versa*. This may be seen by interchanging the names "1" and "2" in the second example: then the two examples will be seen to be identical. Since the names assigned to elements are irrelevant for consistency proofs, the two examples have the same properties. In general, two examples will have the same properties when and only when any systematic interchange of names of elements in one yields a statement identical with the statement of the other. In such cases, the two statements will be taken as different means of designating the same example.

By means of this conception of a field, it is possible to treat abstract systems in extension in a manner strictly analogous to the treatment of classes. An abstract system may be defined as a class of examples belonging to the same field. In this treatment an example corresponds to an individual, the field of examples to the universe of discourse, an abstract system to a class, a theorem of the system to a characteristic of the class, and the set of postulates for the system to the defining characteristic of a class.

Since an abstract system is ordinarily conceived as a group of postulates and theorems, it must be shown first of all that the above view gives some place to postulates. According to the view of an abstract system as a class of examples, postulates are the defining characteristic of the class, and the problem is to show that it is possible to formulate a set of postulates, *i.e.*, give a defining characteristic, in the case of every system. This problem is analogous to the problem of finding an intension for any class defined in extension.

In the case of a single example of finite number of elements, there is no difficulty in finding a postulate which is satisfied by the example and by no other example of the field. All that is required is the substitution of apparent variables for the singular propositional functions of the example and the specification of the number of elements.¹ With any finite number of such examples, a disjunction of the postulates for each example will constitute a postulate satisfied by that system and that system only. In the case of examples containing an infinity of elements, or a system containing an infinity of examples, the case is more difficult. Here postulates of infinite length would seem to be required, and postulates which begin at one end of a blackboard and go on forever are the stuff of which logicians' nightmares are made.

This difficulty of postulates of infinite length, is, however, more apparent than real, since every example in which anyone would be interested can be described by postulates of finite length. The reason for this lies in the obvious fact that, in an infinite example, the holdings or failings of the relation for each permutation of elements cannot be enumerated. The example must be given in some other way; generally by postulates which it alone satisfies or by rules of construction from which postulates may be derived. There is an analogue to this situation in the case of classes. If Zermelo's axiom be granted, there are some classes whose defining characteristic must be of infinite length, but no one is interested in these, and all the classes with which one wishes to deal are describable in a finite number of words. Similarly, though the rule for formulation of postulates suggested above may require infinite postulates in some cases, these are not postulates by means of which one will be called upon to deduce theorems.

¹ Thus the postulate uniquely satisfied by the three-element example given above is:

$$\mathfrak{A}(abc) \therefore a \neq b. b \neq c. a \neq c: Rab. Rac. Rbc. \sim Raa. \sim Rba. \\ \sim Rbb. \sim Rca. \sim Rcc: (d). d = a. \vee d = b. \vee d = c$$

The definition of an abstract system as a class of examples, then, gives a place to postulates by showing that for every system there is a set of laws satisfied by that system and that system alone. It remains to show that theorems are possible, that is, to show that systems as here defined are deductive. To do this, it is necessary to show that all the laws of a system are deducible from the laws satisfied by that system and that system alone.

At this point it is necessary to introduce an hypothesis which seems plausible, but for which there seems to be no proof. The hypothesis is as follows: If any law of an abstract system—and by law is meant either a postulate or a theorem or a group of these—is consistent, it is a law of some example of its field.

While no formal proof of this hypothesis seems possible, the following considerations may serve to make it plausible. Every law imposes a requirement on some or all of a group of elements. Thus the postulate of reflexivity requires that in the case of any element of a group obeying the postulate, the relation holds between that element and itself. Now if the requirement made by a postulate is a possible requirement, there ought to be some conceivable group of elements which obey it. But the field of a relation contains all conceivable groups of elements having to do with that relation. And since a possible requirement would appear to be the same thing as one which is self-consistent, this may justify the assumption that every consistent law is a law of some abstract example.

Suppose, then, P be any consistent set of postulates which is satisfied by all the examples of a system and by no others. Suppose, furthermore, there is some law of the system Q which is not satisfied by all examples of the field. To prove that Q is deducible from P . A simple *reductio ad absurdum* is sufficient for the proof. Suppose Q is not deducible from P , then P must be consistent with not- Q and so " P and not- Q " must be self-consistent. By the previous hypothesis there must be an example satisfying " P and not- Q ". This example, since it satisfies not- Q cannot satisfy Q , and since Q was given as a law of the system, the example which does not satisfy it cannot belong to the system. But the example which satisfies " P and not- Q " must also satisfy P , and P was given as a set of postulates satisfied only by examples of the system. Hence this example must belong to the system. Thus the same example does and does not belong to the system, and this absurdity is sufficient ground for rejection of the supposition that Q is not deducible from P . Hence any law of the system is deducible from the postulates, and the deductive character of abstract systems is established.¹

¹ A complete formulation of the concept "deducibility" is beyond the scope of the present paper. The above theorem has been stated, therefore, only for the cases in which P is not an impossible postulate and Q is not a tautologous theorem, to avoid disputes as to whether an impossibility implies anything and anything implies a tautology. Whether or not the theorem holds in the remaining cases is not a matter of great importance.

Having shown that the definition of an abstract system as a class of examples is in accord with ordinary usage concerning the deductive character of systems, it remains to enumerate briefly a few advantages of this definition.

(1) Consistency proofs may be justified on the principle that abstract examples are necessarily consistent, and that, therefore, a system satisfied by an example, is a class of examples containing at least one consistently defined member. Hence its defining characteristic, *i.e.*, its set of postulates, must be consistent. It is thus possible to explain consistency proofs without recourse to concrete exemplification, and so to justify methods commonly employed in postulate theory.

(2) Independence proofs, involving as they do a proof of consistency, may be explained similarly.

(3) Systems may be genetically defined, by giving rules for the construction of the examples of a system rather than by stating their characteristics. That such procedure is possible is of course a fact, but it seems difficult to justify this possibility on any other view of abstract systems.

(4) A set of postulates is commonly held to be complete or categorical when at least one of every pair of contradictory laws concerned with the same variables as the postulates is deducible from them.¹ In terms of the suggested definition of abstract systems, this reduces to the requirement that the system contain at most one example.² Thus completeness may be accounted for in terms of the definition, and a mode of establishing completeness, however difficult of application, is suggested. More important, it is possible to account for one interesting form of partial completeness—that in which the system is complete except for the specification of the number of elements it contains. This reduces to the requirement that it contain at most one example of any given cardinality.

¹ Cf. Lewis and Langford, *Symbolic Logic*, p. 351.

² For every example, it has been shown, there is some law which is satisfied by it alone. Suppose then a set of postulates defining a system of at least two examples and let a and b be two of these. Let A be the law satisfied uniquely by a . Then $\sim A$ is satisfied by b . Neither A nor $\sim A$ can be deducible from the set of postulates, for if A were deducible, b could not belong to the system, and if $\sim A$ were deducible a could not belong. Hence no system of more than one example can be complete.

That every system of a single example is complete depends on the fact that an example satisfies one of every pair of contradictory laws, just as an individual is a member of one of every pair of complementary classes.

PAUL HENLE.

ARTISTIC FORM AND THE UNCONSCIOUS.

SINCE Dr. Jones has kindly intervened to order our confusion and lighten our darkness, I must, even at the risk of disturbing the sense of finality, ask him for some more of that order and light of which I admit I stand badly in need. In the paper to which he refers I allude slightly only to the ultra-medical applications of the theory of the Unconscious. For the medical application I have a considerably greater respect, based on what I had hoped was a less "tenuous apprehension" of it because it is itself less tenuous. Even though I did not say so in my paper, I do not forget that the ten senses in which "the Unconscious" is used in these applications "are all invented in an armchair" (not, however, in my armchair). Is not Dr. Jones' own application of the theory to Shakespeare's *Hamlet* an "armchair" one? Or does he claim to have summoned Shakespeare's spirit from the vasty deeps and to have tested his imaginative processes in the laboratory? Alternatively, does the curing of many psycho-neurotic patients, effected or presumed to have been effected with the help of the theory of an incest-complex, itself constitute an energetic or pragmatic, unsedentary, *solvitur ambulando* kind of proof that Shakespeare's play is the fruit of an incest-complex? I should have thought that the only "proof" possible in this case was that the behaviour of the imaginary character Hamlet *might* be diagnosed as symptomatic of an incest-complex in him (possibly, but not necessarily, also in Shakespeare) *but also of any one of many other alternatives*. If this is not "armchair" work, then what is?

My very genuine difficulties with the "Unconscious" relate not to definition but to use. I can formulate them in two questions, and if Dr. Jones answers these he will have rendered me a signal service by making my apprehension less tenuous and my temper one of humble gratitude instead of "dictatorial."

(1) A philosophic, scientific, historical or psycho-analytical theory may be said to issue from an "Unconscious," in the sense at any rate that before it has become a finished product the producer himself cannot envisage it, is not properly aware of it, while it is nevertheless operative within him, evolving itself, guiding his selection and rejection which constitute the process of production, assembling, so to speak, its parts. The same may undoubtedly be said of a poem or any other artistic product. But can or must a connection between art and the "Unconscious" be predicated *sensu eminentiori*? (The theory, mentioned by Dr. Jones towards the end, that the creative impulse is a substitute for, or modification of, other urges cannot differentiate art since the creative impulse

is not peculiar to it. Moreover, sublimational, substitutional or Changeling psychology may be Freudian, but it surely is not the only "scientific" psychology. Individualist or Adlerian psychology also deserves a hearing. Dr. Jones speaks as though his psychology were the only claimant in the field.)

(2) The "Unconscious" from which creation comes is presumably healthy and dynamic. The "Unconscious" with which psychoanalysts deal therapeutically is *ex hypothesi* unhealthy and would seem to be mechanical. Why should the two be lumped together as "the Unconscious"?

I said in my paper that the psycho-analytical "Unconscious", the "Unconscious" of mnemonic residua, though it did not explain artistic creation or meaning, might supply the artist with some of his matter. But the artist's use of it, I maintained, was precisely to bring it into consciousness. *Therefore* (strange *igitur*!), says Dr. Jones, I was referring to what is "really conscious" (i.e., presumably, to that of which we are aware before the artist, by the sweat of his brow and the might of his genius, has dragged it up into the light) "or at least readily accessible to consciousness". I meant and mean, quite frankly but also, I hope, quite inoffensively, that Shakespeare, because of his special genius, is better at the job of bringing up the unconscious (especially his own) into consciousness than any psychoanalyst. In this I am borne out by Jung and Adler who bring imaginative writers into the court of Psycho-analysis not in the way in which Dr. Jones seems to do, as patients or *vilia corpora*, but as masters and experts. Dr. Jones would seem to hold that the artist's special business is with the Unconscious. Presumably it is to bring the latter into consciousness. But this, it appears, the artist cannot do; only the psycho-analyst can do it. It would seem to follow, therefore, that Dr. Jones must hold that Psycho-analysis should replace art. This is being dictatorial with a vengeance!

Dr. Jones is probably right in thinking that my conception of the psycho-analytical "Unconscious" is different from his, and mine is therefore very likely wrong. But from what he says I cannot quite gather where mine differs from his. Still, I fix upon the following as significant: The unconscious (in the orthodox sense as contrasted with mine) is that of which the subject is "absolutely unconscious". It is "dissociated from the conscious self". "Only derivatives" of it, "much distorted and modified, can reach consciousness" (even with the help of the psycho-analytical technique?). Will he accept the following further elaboration of his meaning? Even to the patient himself and even with the help of the psycho-analytical technique, a content of his unconscious is never accessible as an object of acquaintance, but always only as an inferential construct (in the way, for example, in which Caesar's crossing of the Rubicon or his toothache is accessible to me), a construct presented in a verifiable but not provable hypothesis. For such access and presentation what is needed is not the penetrative imagination of a Shakespeare but the inductive and deductive reasoning of the

scientist. Further, what is relevant to the patient's cure is not so much the content of the explanation of his state offered to him, as his conviction that an explanation has been found; the content is as immaterial as the words of a magic formula.

The conception which I have had up to now at any rate about the same "Unconscious" I can best describe as follows: A content of the patient's "Unconscious" is a present trace,¹ *unbewusst* or unbeknown to him² but in him, of what he or his ancestors were once acquainted with: *e.g.*, an incestuous impulse (experienced without, of course, being described or conceived as incestuous) in his own infancy or in his remote marine or arboreal ancestors (hence my use of the term "mnemic residuum"). With the help of the psycho-analytic technique, but above all with the help of intuition and sympathy like the artist's, the original object of acquaintance, as well as its present trace, can once more be brought to his acquaintance (such re-acquaintance we have in recollection), and he can be helped to grapple with them, as well as with their effects, "bodily". Such an "Unconscious" is of course not absolutely unconscious nor absolutely dissociated from the conscious self but continuous with it.

I am very ready to be corrected on this, as my sole acquaintance with medical psychology is through armchair reading.

Whether Dr. Jones has really grasped what was the matter with Shakespeare when he wrote *Hamlet* I still venture to doubt. But he has certainly understood what was the matter with me when I wrote my paper, and is still the matter with me. I do believe in "uncaused mentation". At least I believe that the category of causation is very inadequate and even misleading for the description of perception, knowledge, imagination and intelligent action. But I can shelter myself under the aegis of a very successful psycho-analyst, F. Kunkel,³ who works with the notion that there is no caused mentation whatsoever. Dr. Jones objects that "the conception of 'uncaused mentation' is one with which no scientific psychology can operate". If scientific psychologising means the unseasonable aping of the notions of an archaic physics, all I can say is "*Je n'en vois pas la nécessité*". I am content to follow, not the "scientist", but Aristotle's educated man who seeks for those methods only which are appropriate to each particular sphere. But is not the greatest service that Psycho-analysis can render in the ultra-medical sphere precisely to rid us therapeutically of the minatory maxim "Science is impossible if . . ." ? It is the formula of the dictatorial *Dressat* of a well-known psychosclerosis—better known, surely, to Dr. Jones than to myself.

¹ Or it may be not a trace but the original unchanged.

² *I.e.* he is not acquainted with it as he is, say, with his present sensation of cold, however much he may know *about* it by reading, for example, psycho-analytical descriptions.

³ See *Vitale Dialektik, Einführung in die Charakterkunde, and Charakter, Leiden und Heilung*.

V.—CRITICAL NOTICES.

Problems of Mind and Matter. By JOHN WISDOM. Cambridge : at the University Press, 1934. Pp. xv, 215. 6s.

'In this book', Mr. Wisdom writes, 'an attempt is made to give an elementary but not too inaccurate introduction to the applications in philosophy of what is now sometimes called the analytic method.' (Preface.) And he goes on 'This might have been done by explaining the nature of analysis, but here little is said *about* analysis, and instead elementary examples of its *use* are given'. The examples which Mr. Wisdom chooses are two of the most important relations between mind and matter, that of owning or animating and that of cognition, mainly perceptual cognition. There is, however, a short introductory chapter which tells us briefly what analysis is. The rest of the book naturally falls into two main divisions. Part I. discusses the relation of body and mind, and concludes with a chapter on Free Will. Part II. discusses perception and the external world, and concludes with a chapter on Judgement and Truth. There are also two short appendices, one on the relation of Intentionality, the other on Universals and Particulars, and an index.

The book professes to be no more than an introduction. But this modest claim must not deceive us into thinking that only beginners in philosophy need read it. It is indeed brief, and it is written with admirable clarity, as an introduction should be. But it contains a number of novel and interesting doctrines, and indeed has something new to say on each of the main topics that it touches. In all these respects it is a fitting successor to another Cambridge book, Mr. Russell's *Problems of Philosophy*. And as the dust-cover indicates, it is rendered more valuable by its discussion of several important but elusive theories of Prof. Stout's.

I shall now give a brief account of the course of Mr. Wisdom's main argument, pausing from time to time to offer some comments and criticisms.

He begins (Introduction) by telling us what analysis is and how it differs from speculative philosophy. The aim of analysis, he says, is clarity, whereas the aim of speculative philosophy is truth. He then proceeds to his first examples of analysis, the definitions of 'history', 'science', 'mental facts' and 'material facts'. We are then given an Analytic Vocabulary. Starting with the notion of a fact, we distinguish two sorts of elements in facts, *viz.*, *components*

and *constituents*; components being always universals (of quality or of relation), while constituents may be either particulars or universals. We also distinguish between *complete* or elementary facts such as 'this is black', and *incomplete* facts (or rather complete facts incompletely stated) such as 'something is black'. Finally, we distinguish between *facts* and *events*, a fact being according to Mr. Wisdom an infinitely thin temporal slice of an event.

This last suggestion, about facts and events, is one of the most interesting in the book. But there seem to be several difficulties in it. First, if there are 'complete' facts whose constituents as well as components are universals (as Mr. Wisdom says there are on p. 27) it is clear that *they* cannot be infinitely thin slices of events. He ought to say that such facts as 'orange is between red and yellow' are incomplete or non-ultimate, in the same sort of way as 'something is red' is incomplete. Secondly, is it not odd to say that a fact is *at a time*, or even *happens at a time*? Suppose I began sneezing at exactly 4 p.m. yesterday. Should we say 'at 4 p.m. yesterday it was a fact that I began sneezing'—and now, presumably, it is not a fact any longer? Clearly we should not. We should say 'it is a fact that I began sneezing then' using the 'is' in a timeless sense. Thirdly, in Mr. Wisdom's view there can be no ultimate facts whose components are such sensible form-qualities as tick-tockness or tickliness or visible motion; for these form-qualities take a finite time to be manifested. But surely such facts as these are elementary or complete if any are? Lastly, should not Mr. Wisdom say that facts are infinitesimal in space as well as in time? But if so, it seems doubtful whether even such a quality as cherry-redness can be a component in any complete or elementary fact, for presumably it can only be manifested in a finite area. Indeed I do not see that there are *any* characteristics which can be components in elementary facts on this theory, except spatial and temporal position. And this is a very unpalatable conclusion.

However, it would clearly be an advantage if we could reduce facts to events or *vice versa*. It is incredible that the world should contain *both* facts and events, in the sense in which it contains both cats and dogs. So far Mr. Wisdom's attempt is praiseworthy. But since the whole point of introducing facts is to provide something for propositions (or beliefs) to accord or discord with, would it not be better to say that a fact is an event or set of events in its capacity of making propositions true or false? Thus 'it is a fact that A is B' might be equivalent to 'there is an event or set of events, such that the proposition *A is B* is true, and *A is other than B* is false'. Such a theory would require a great deal of elaboration, and perhaps it might break down when we considered the various sorts of facts which have to be provided for. But it seems a more promising kind of theory than Mr. Wisdom's.

We may now turn to the discussion of Mind and Body in Chapters I.-VII. Mr. Wisdom is concerned with only one of the

many relations between them, namely, that of *ownership*. His question is, What do we mean by saying that a certain mind 'owns' a certain body, or rather (as he adds in a footnote¹) what do we mean by saying that a person 'owns' both a certain mind and a certain body? And even this question he does not claim to discuss fully. He confines himself to asking what part, if any, the causal relation plays in the analysis of ownership. Chapter II gives an elementary account of the human nervous system, in order to make clear the distinction between nervous events and mental events. (It is surprising how many intelligent people fail to distinguish them.) Chapter III. is concerned with Materialism. Simple Materialism, which just denies the existence of mental events altogether, is summarily dealt with; since if it is *believed* to be true it cannot be true, for believing is a mental event. But there is also Analytic Materialism, or Behaviourism, which admits the existence of mental events but holds that they can be analysed into material events inter-related in a complex way. This is refuted by the following argument, borrowed from Prof. Broad: Behaviourists hold that 'this body has a mind' is equivalent to 'such and such movements, etc., are occurring in this body'. But even when we know that these movements, etc., are occurring in a certain body, it is *still* sense to ask 'has it got a mind?' If Behaviourists are right it could not still be sense to ask this.

The way is now clear for Mr. Wisdom to discuss his main question, whether there are causal relations between mental and material events. But he holds that the word 'cause' is ambiguously used in ordinary speech, and accordingly he draws a distinction between *occasioning* and *producing*. This distinction seems to be the same as the ordinary one between part-cause and whole-cause. That which 'produces' A or 'is the total explanation of' it (Mr. Wisdom says that these expressions are synonymous) is that which is both necessary and sufficient for the coming about of A. This total explanation will contain both events and persisting states. Then the events which it mentions are said to 'occasion' A. Thus we have first to ask whether bodily events ever *occasion* mental events or *vice versa*; if they do, we can then ask whether bodily events (together with persistent bodily states?) ever *produce* mental ones or *vice versa*.

This language seems to me rather unfortunate. For 'occasioning', especially in this context, cannot but suggest the doctrine of Occasionalism. But when the Occasionalists said that bodily events occasion mental events and *vice versa*, they meant just what Mr. Wisdom does not mean; they were asserting, for instance, that a bodily event is never part of the cause of a mental one. Mr. Wisdom's argument would have been much clearer if he had substituted some phrase like 'being an occurrent cause-factor' for 'occasioning'.

¹ P. 38.

To return : Mr. Wisdom proceeds to give a number of arguments to show that mental events are cause-factors in the bringing about of bodily events, and *vice versa*. His main ones are derived (1) from the intimate and detailed correlation of mental and bodily (nervous) events, (2) from inspection. Under the second head he even maintains that a man who sees a pin stuck into him can know by inspection that the entrance of the pin is a part-cause of the pain which he feels. This situation, he thinks, is quite unlike that in which we know by inference that muscle-strain in our eyes (for instance) is a part-cause of our headache. As regards mind-body causation, he examines Hume's argument from the plain man's ignorance of the nervous system. (Surely this argument, for what it is worth, applies to body-mind causation just as much ?) He admits that a volition is never a *sufficient* cause of the bodily movement which we are said to will. And he thinks—here differing from Prof. Broad—that we cannot know by inspection that it is even a *necessary* cause of the movement. But he nevertheless claims to know by inspection that there *is* a causal relation of some kind between the two. With regard to the objection derived from the Principle of the Conservation of Energy, he replies—rightly, I think—that the objector is assuming that *all* causation involves transference of energy. Now this proposition is not self-evident. It could only be established empirically, if at all. And anyone who sets out to establish it must first examine instances of psycho-physical causation. But if so, he will have to admit that there *are* instances of psycho-physical causation, and so the objection collapses.

Bodily events, then, do often 'occasion' mental ones, *i.e.* are part causes of them, and *vice versa*. We have now to ask whether bodily events ever 'produce' mental events or *vice versa*; or again whether events of the one kind are ever 'the complete explanation of' events of the other kind. Mr. Wisdom answers, No. And here, like other philosophers who have discussed this problem, he bases his answer on certain general principles about causation.

He starts from the familiar but vague statement that mind is too unlike matter for either to influence the other. But what does this mean? To elucidate it Mr. Wisdom refers us to a doctrine of Prof. Stout's that one situation can only be the complete explanation—or 'producer'—of another if there is a generic likeness between them (of course there may still be great unlikeness of detail). It follows, according to Prof. Stout, that no mental event can be the complete explanation of a material event, and no material event of a mental one; for there is not the requisite generic likeness between them. The principle here appealed to Mr. Wisdom calls the *Principle of Generic Resemblance between Cause and Effect*. His own formulation of it is: if S explains S', then they must be manifestations of the same supreme variables (p. 89). But he holds that there is another principle about production, not to be confused with this one, which he calls the *Principle of Continuity*. This concerns likeness in another

dimension, so to speak, namely, likeness of cause to cause and of effect to effect. It states that generically like causes produce generically like effects; thus, to take an example of his own, if a speed of 10 m.p.h. causes wind-resistance, then a speed of 20 m.p.h. will also cause wind-resistance. This principle then does not state that effects are similar to their causes, as the other did; but only that the effect of one cause is under certain conditions similar to the effect of another. (Why does Mr. Wisdom call this latter principle the Principle of *Continuity*? The name suggests that he is asserting that there is no spatio-temporal interval between a cause and its effect. But this, be it true or false, is not at all what he is saying. 'Principle of Correspondence' would be better.)

Mr. Wisdom appears to hold that the Principle of Continuity is self-evident. But he hesitates a little over the Principle of Generic Resemblance. He seems to think that it is not evident at first sight, but becomes so on reflection. He admits that it is not logically necessary, i.e. that the denial of it is not self-contradictory. But on reflection it turns out, he thinks, that 'we somehow know that this world is a causal system having generic continuity' (p. 95).

Mr. Wisdom concludes from these considerations that bodily events do not produce (or completely explain) mental ones, and mental events likewise never produce bodily ones. But, he adds, it does not follow (as Prof. Stout thinks it does) that bodily events do not even *occasion* mental events, nor mental events bodily ones. Thus we can avoid accepting Prof. Stout's puzzling doctrine that the relation between the two kinds of events is not a causal but a 'logical' relation, where mental events and bodily events somehow complete each other, in the same sort of way as the fact that this is red completes the fact that this has size. All that the Principle of Generic Resemblance requires is that *part* of the total cause (or effect) of a mental event must be mental, and part of the total cause (or effect) of a material event must be material.

We now return to the relation of ownership. Can we say with the ordinary Interaction Theory that a mind and a body stand in this relation when each occasions changes in the other (in Mr. Wisdom's sense of 'occasion')? Not quite. Here again Prof. Stout is appealed to; he maintains that when A interacts with B there is always some *other* relation between the two besides that of interaction, usually the relation of spatial proximity. Mr. Wisdom accepts this by no means obvious proposition. What then can this further relation be in the case of mind and body? Mr. Wisdom suggests, with fitting hesitation, that consciousness may somehow or other be spread out all through the body, so that this other relation would be that of spatial co-presence. I do not know whether this is the doctrine of the schoolmen that the soul is *non in loco sed ubi*. Whether it is or not, it seems to require a good deal of clarification. Certainly it is attractive to common sense. But this I think is only because organic sense-data are voluminous; and since they are very

closely connected with our emotions, we tend to assume that we ourselves, the subjects of the emotions, are voluminous too. But this conclusion does not follow. Further, if the mind is voluminous, what is to become of the unity of consciousness? Shall we not have to say that the mind, although extended, somehow avoids having *partes extra partes*? However, I cannot myself see any conclusive reason for accepting Prof. Stout's doctrine that *wherever* there is interaction, there is some further relation between the agents, unless numerical otherness be one. This proposition is not self-evident, and on the face of it only appears true where for 'interaction' we substitute 'interaction between two material objects', in which case it is of course irrelevant to our present question. In fact Mr. Wisdom's objection to the Conservation of Energy argument above would apply just as well to this argument of Prof. Stout's.

The last chapter of this part of the book discusses Free Will. In it Mr. Wisdom reaches the surprising, but on reflection not unpalatable, conclusion that blameworthiness entails pre-existence. He argues that I cannot be blamed for a certain voluntary act which I have done unless every volition of mine has been partly determined by some previous volition of mine; and consequently the series of my volitions must extend backwards in time before my birth, and must be 'either infinite or world-long'.

There are a number of points in these chapters which call for further comment. I shall confine myself here to Mr. Wisdom's views about causality. In the first place, he holds—as we saw—that we sometimes know by inspection that one particular event causes another particular event; for instance that a particular pin-prick causes a particular pain. Or again, I might know, he thinks, that I now feel annoyed *because* I heard Jones call me a fool two minutes ago. Secondly, he holds that three general causal principles are certain: *viz.* the Principles of Generic Resemblance and of Continuity and the principle that every event has a cause.

All these contentions seem to me very questionable. First, it is not at all clear to me that particular psychophysical causal connections are ever known by inspection. So far as I can see, it is just an inductive generalisation that whenever a pin is stuck into my body I shall probably feel a pain. Doubtless there is a great deal of evidence for this generalisation. Moreover, if ever I did not feel a pain in such circumstances, I should certainly be very much surprised; for the thought of having a pin stuck into my body is very firmly associated in my mind with the thought of feeling a particular sort of pain. But so far as I can see that is all. Nor can I see that the situation is relevantly different in the case of volitions and bodily movements. I find that when I will to move such and such parts of my body they usually do move, and I expect that they will do so again. It is true that here I am often directly aware of another relation besides the mere sequence; *viz.*, of a relation of coincidence (if it may be called so) between what is willed

and what actually comes off. For instance, what I will is that my mouth should open, and my mouth does open. But this relation of coincidence, though it certainly does seem to differentiate the case of voluntary movement from all other observed regular sequences, is by no means that 'mysterious bond' which Hume looked for and could not find. I do not of course venture to maintain that Mr. Wisdom and the other distinguished philosophers who say that they do here observe the mysterious bond are certainly mistaken. It may well be that I have not attended to the situation with sufficient care. But I cannot at present see that they are right.

To support his view Mr. Wisdom quotes a passage from Prof. Broad,¹ to the effect that those who share these doubts or this blindness 'must hold that the first time a baby wills to move its hand it is just as much surprised to find its hand moving as it would be to find its leg moving or its nurse bursting into flames'. But why should not this be held? If one is absolutely new to this world is not every matter of fact surprising? Or perhaps it would be more correct to say that nothing is either surprising or unsurprising; for perhaps an event can only be surprising if it conflicts with some inductive generalisation which one has already made, and presumably the infant in question has not yet made any.

The most plausible case on Mr. Wisdom's side, however, is that of purely intra-mental causation. But do I really see that hearing this particular remark causes this particular feeling of annoyance in me? One difficulty is that the hearing of such remarks as this is *not* followed by annoyance in all men (*e.g.*, not in Christian or Buddhist saints). And even in ordinary mortals the sequence is perhaps not invariable. 'But that is because our state of mind is different. We only claimed to know that hearing the remark was a part-cause of the annoyance, not that it was the whole cause.' Very good: but this admission, like the corresponding one with regard to psycho-physical causation, greatly weakens the case. It is now admitted that the whole of the cause is not necessarily known by inspection; part of it may be mental but 'unconscious' (*i.e.*, unintrospectible) and part of it may, of course, be purely physiological. But is being a *part-cause* of something a characteristic which could possibly be known by inspection in such a case as this? Can I *know by inspection* that A is a part of some whole when I do not and cannot know the other parts by inspection? How am I to know that there is such a whole at all? Of course I may well *believe* that there is. But if I do, it is only on inductive grounds, and such beliefs are obviously fallible.

I think, however, that a plausible reason can be suggested for Mr. Wisdom's mistake—if it is one. If asked 'why are you annoyed now?' I should often answer '*because* I heard such and such a remark two minutes ago'; and in answering so, one does seem to be describing something known by inspection. But the '*because*',

¹ *The Mind and its Place in Nature*, p. 102.

I suggest, is not really that of causality. What I know by inspection is that my annoyance is directed on a certain *object*. In other words, the question which I am really answering is 'what are you annoyed at'? And the answer to this can be known by inspection.

We may now turn to Mr. Wisdom's three general principles about causality. First there is the principle that every event has a cause. I have to confess that I cannot on reflection find this to be self-evident; nor does there seem to be any satisfactory way of proving it. All that I can see is that I habitually take its truth for granted.

What of Mr. Wisdom's other two general principles (which are, of course, logically independent of this first one)? The second, the so-called Principle of Continuity, does seem plausible. As Mr. Wisdom says, it follows from the generally admitted maxim that like causes produce like effects. But why is this latter maxim generally admitted? Because it amounts to saying that causes work according to *laws*, or that where there is causation *generalisation* is possible. Now on the Regularity Theory of causation, this will follow analytically from the notion of cause itself. But will it on other theories, for instance on Mr. Wisdom's own? It is clear that he is bound to reject the Regularity Theory, since he holds that he can inspect causal connections between individual events. Therefore on his view the maxim 'like causes produce like effects' is not a *logically* necessary proposition, *i.e.*, the denial of it would not be self-contradictory. It is not, therefore, clear that *he* is bound to admit this maxim (though holders of the Regularity Theory certainly must) or indeed that he has any right to admit it. If causes are what he suggests they are, the proposition that they work according to laws will need proof, and it is not at all clear what proof could possibly be offered.

Lastly we come to the Principle of Generic Resemblance, perhaps the most interesting of the three. This states that if S completely explains S', then they must both be manifestations of the same supreme variable. Stated thus, this principle appears very plausible. If two situations were regularly correlated but were not manifestations of the same supreme variable, then though we could still *predict* the second given the first, we should, I think, feel that the second was not completely explained by the first. Consider for instance the law that if any thing gives off light-rays of such and such a frequency, it looks blue to the normal eye. We all feel that there is something defective about this statement. There certainly is a sense in which the emission of the light-rays does not completely explain the blue-lookingness. But is there not an ambiguity in the phrase 'completely explain'? Mr. Wisdom uses it as a synonym for 'produce' and 'be the complete cause of'. Now this, I think, is not the ordinary sense of the phrase at all. In the ordinary sense of it, a complete explanation is one which satisfies the intellect;

it is one on hearing which we no longer ask 'why?' Now if the law *when S then S'* is to satisfy the intellect, I agree that S and S' must be manifestations of the same supreme variable. But do they have to be so if the law is merely to be a 'complete explanation' in Mr. Wisdom's *non-ordinary* sense, of stating the complete cause? Unfortunately it is not clear that they must be. How can we be sure that all complete causal laws (if I may call them so) must be such as to satisfy the intellect? So far as I can see we cannot be sure at all. We may hope that they will, and this hope may be a powerful motive to make us search for complete causal laws when we have not found them; but I see no guarantee that our hope is bound to be fulfilled. If, as we are told, the laws of Mechanics come near to fulfilling it, is this more than an uncovenanted mercy?

It seems possible that Mr. Wisdom may have accepted the Principle of Generic Resemblance in the first place because he began by taking the phrase 'complete explanation' in its usual sense (explanation satisfactory to the intellect). But when he comes to apply it he uses the phrase in his own unusual sense (complete cause). And then the Principle becomes something quite different, and something not by any means obviously true, though we still cannot say that it is certainly false.

We may now turn to Part II., which is entitled 'Cognition', and first to Mr. Wisdom's discussion of Perception and the External World. I shall not discuss this in so much detail. It seems to me to be very well done, and the topics dealt with are skilfully selected. But I do not think that it is so exciting or so controversial as Part I.

Mr. Wisdom's first question is, what is the analysis of a normal perceptual situation, such as occurs when I say truly 'I see a penny' or more accurately 'I see the top surface of a penny'. He answers: (1) I am sensing a certain sense-datum as having a certain sensible quality; (2) This sense-datum stands in a peculiarly intimate relation to the top surface of a certain penny; to indicate this fact, it is called *the corresponding* sense-datum, because no other sense-datum sensed by me at that time is thus related to that surface. This leads to two further questions: (a) What is the relation between the percipient, the sense-datum, and the sense-quality or qualities: *i.e.*, what is the analysis of 'A senses S as having C'? (b) What is the relation between the corresponding sense-datum and the penny?

As to (a) Mr. Wisdom suggests two alternative views. The simpler one says that sensing S as brown is equivalent to discovering that S is in fact brown. The more complex one says that facts involving brownness and other sensible qualities are not two-termed but three-termed, the third term being always a percipient mind; in which case the statement 'this is brown' is always incomplete (like 'this is disgusting'), and the full statement is 'this appears brown to someone' or 'this is sensed-as-brown by someone'. It will follow, as Mr. Wisdom points out, that such facts are *logically* and not merely causally mind-dependent.

Thus we are confronted with a new question: 'Do sense-data really (*i.e.*, intrinsically) have the qualities which they are sensed as having, or do they merely appear to have them'? (p. 149). According to Mr. Wisdom, the familiar facts as to perspectival distortion make it certain that either the corresponding sense-datum is *usually* not identical with the surface of the object, or the qualities which it is sensed as having are *usually* apparent, not intrinsic.¹ But are we bound to conclude from 'usually' to 'always', or from 'usually not' to 'never'? Most philosophers have in fact done so. But, as Mr. Wisdom points out, the step is not logically necessary. There might be *optimal* sense-data, such as the one which a man senses when he observes a penny head on, from a short distance, in a good light, with a normal eye, etc. And so far it is logically possible that such an optimal sense-datum might *both* be identical with part of the surface of the object, *and* intrinsically have (not merely appear to have) the qualities which it is sensed as having.

Mr. Wisdom, however, thinks it unplausible, on grounds of continuity, to say that some sense-data are identical with material surfaces and some are not. Could we say then that all are, both the optimal ones and the perspectified ones? Mr. Wisdom answers that perspectified ones could be identical with material surfaces, provided that the distorted shapes which they are sensed as having are apparent and not intrinsic. If a sense-datum is only *apparently* elliptical, it may at the same time be *intrinsically* circular; and so it may after all be identical with a circular material surface, in spite of being 'sensed as' elliptical.

But what is to be done about dreams, and what about seeing double? In all these cases we have a sense-datum which on the face of it cannot be identical with the surface of any material object, however much its intrinsic qualities may be supposed to differ from its apparent ones. (There would be a similar difficulty with hallucinations). With regard to dreams, Mr. Wisdom suggests—following Prof. Dawes Hicks and others—that the sense-data may be identical with parts of the surface of the sentient's brain; the sentient of course would not himself realise that they were identical with these, but still they might in fact be so. With regard to double vision, Mr. Wisdom suggests—again following Prof. Hicks—that the two sense-data, though not identical with the surface of the candle (say), may none the less be identical with parts of the surface of something beyond it, say with parts of the surface of the wall of the room. If so, it is perfectly possible that *all* visual (and tactual) sense-data are parts of the surfaces of material objects. And it is also perfectly possible that in *some* cases they do intrinsically possess the qualities they are sensed as having; or at any rate the qualities they are

¹ *I.e.*, either *most* corresponding sense-data are not identical with the surfaces which they respectively correspond to; or the qualities which *most* sense-data are sensed as having are apparent, not intrinsic.

sensed as having may approach more and more closely to their intrinsic qualities as the conditions of observation are improved.

There are several points here which call for discussion. First, with regard to *optimal sense-data*; it is clear, though Mr. Wisdom does not mention this, that we do all call some perceptual situations 'better' and some 'worse'. Thus we say 'you can see *better* from here' or 'Jones does not see so *well* as Smith'. We all attach meaning to this way of speaking, and moreover we usually find no difficulty in deciding *which* perceptual situations are better than which others. Thus we all seem to know what is meant by the phrase 'optimal sense-datum'; and even if we never actually obtain one, we all seem to know when we are getting nearer to doing so. Now Mr. Wisdom suggests that an optimal sense-datum could be defined as one sensed from the most favourable position, with a healthy eye, in a good light, and so on. But to this definition we may object that as soon as we try to apply it we are involved in an infinite regress. How am I to discover that my eye *is* healthy, that my position *is* such and such, that the light *is* good? Only by observation, if at all; that is, by sensing other sense-data. One does not learn these things by immediate revelation from Heaven. And if I am to be sure that my eye is healthy, I must be sure that the sense-datum by means of which my eye is observed (whether by myself or my oculist) is itself an optimal one, or at least is near enough to being so. Otherwise it may be that my eye only *seems* to have the requisite shape or the requisite disposition of parts or what not. There is just the same difficulty about the other conditions. How, for instance, am I to tell what my position in space is? Again, only by means of other sense-data; and these again must be optimal ones, or near enough to optimal, otherwise I can only be sure that I *seem* to be two feet away from the penny and vertically above it, not that I really am so.

It seems to me that the only way out of this difficulty is provided by what I have elsewhere called the Theory of Families of Sense-data. Roughly speaking, a sense-datum is an optimal or normal or standard sense-datum when it is *spatially synthesisable* with other sense-data in such a way that the whole set forms a complete three-dimensional surface. Now if I view a penny from two feet away, the visual sense-datum which I sense *is* as a rule spatially synthesisable with other earlier and later visual data, say with those which I get when I turn the penny round in my hand. And as for the normality or abnormality of the eye, that surely is inferred from the normality or abnormality (the synthesisability or not) of the sense-data which we sense by means of it, and indeed must be *defined* in terms of this. A normal eye is an eye such that you can sense normal sense-data by means of it; though only a physiologist or oculist could tell us what the detailed structure of such an eye would be. Thus Mr. Wisdom's definition of optimal sense-data puts the cart before the horse, as well as involving a regress. For-

fortunately, however, this does not matter to his main argument, provided that an unobjectionable definition of 'optimal' can be given, as I have just suggested that it can.

There is also a difficulty in Mr. Wisdom's discussion of *dissociated sense-data*, if I may call them so; meaning by this, sense-data which cannot *prima facie* be identical with the surfaces of material objects. He mentions only dreams and double vision. But there are other cases: *e.g.*, all the phenomena of refraction and reflection; the blue sky; hallucinations of all sorts, varying from the reading of printed words which are not there to the seeing of full-blown pink rats or apparitions. Mr. Wisdom may say that he is only concerned with the analysis of ordinary perceptual situations, and so is not bound to discuss these cases. To this I answer, first, that they differ only in degree from the cases of dreams and double vision, which he does discuss; secondly, that they are plainly only too relevant to the analysis of ordinary perceptual situations.

As we have seen, Mr. Wisdom has two expedients for dealing with dissociated data. Either a dissociated datum is identical with part of the surface of the brain: this he applies to dreams and presumably to hallucinations. Or it is identical with part of the surface of the background: this he applies to double vision and presumably to refraction and reflection. Both these suggestions seem questionable. With regard to dreams, the difficulty is that one may be in a state between dreaming and waking, or rather in a state which is partly one of dreaming and partly one of waking perception. This plainly happens in ordinary delirium. Likewise with hallucination. If I see a 'real' page, but see it with some words on it which are not there, or again if I see an hallucinatory rat in the midst of my 'real' hearthrug, my total visual field is partly hallucinatory and partly not. Now it seems difficult to believe that most of my visual field is identical with the surface of a certain hearthrug, while the remaining bit is identical with a portion of the surface of my brain. Is it not very queer that a small tract of my brain should, so to speak, insert itself among the furniture of my room? (Doubtless I shall not *take it* to be a tract of my brain, but the queer thing is that it must actually *be* a brain tract, if Mr. Wisdom is right.) Also we shall have to credit the brain with two entirely different functions in regard to sensation: on the one hand it reveals to me certain extra-bodily surfaces, usually but not always causing them to appear what they are not; on the other, it reveals to me certain surfaces inside itself. And it may happen, as in the case we are considering, that while one brain tract is performing the first function, an immediately adjacent tract is simultaneously performing the second. Perhaps this is not absolutely impossible, but it is very difficult to believe.

Let us now consider Mr. Wisdom's other expedient, that the dissociated sense-datum is identical with part of the surface of the background. I think that Mr. Wisdom only 'gets away with it'

here because he ignores the character of *depth* which visual sense-data have. When I look at a candle-flame two feet away and see it double, it seems to me quite clear that the two sense-data are *sensibly* located in the same plane. It is not that I *estimate* them to be on the same plane: I sense them as being so. (Perhaps an infant or a cat would not, but I do.) Now, if so, one of the flamy sense-data will have to be identical with the surface of a volume of air. It is strange that air should thus become visible and yellow-looking, but perhaps not impossible. But what of a mirror-image? Suppose I stand a foot away from a small mirror hanging on a wall, and look at my face in it. Here again it seems clear to me that the reflection is at a greater *sensible* depth than the sense-data manifesting the glass or the mirror-frame. Shall we say then that the reflection-datum is identical with the surfaces of certain bricks in the wall—inside surfaces at that, which could only be exposed by cutting? It would be queer, but perhaps not absolutely impossible. But suppose that there was a vacuum behind the mirror; e.g., I might look at my face in the polished surface of a large vessel exhausted of air. Then it seems that there is *no* material object with whose surface the sense-datum can possibly be identical. There is only a volume of empty space. So, too, if we see the moon double, as we may well do, or a star, or the sun.

This kind of situation suggests another theory¹ of dissociated data, which I believe Mr. Wisdom would have done better to adopt. Why should he not say (1) that every visual or tactual datum is identical with the surface of some volume of space; (2) that sometimes this volume is pervaded with causal properties markedly different from those manifested on the hither side of it; but (3) sometimes it is not? In the one case, the volume which the sense-datum decorates will be said to be *occupied by a material thing*, in the other case it will be said to be *empty*. (I purposely use the common-sense word 'thing' rather than the technical phrase 'physical object'; for of course there are usually physical objects, e.g., air-particles, in parts of the spaces which common sense calls empty. A thing is a physical object or complex of physical objects whose perceptible properties are such that it is sharply differentiated from its surroundings.)

Now there is no offensive discontinuousness about this theory. All extensive sense-data will be alike in being identical with surfaces of volumes of space. Some of these volumes will be occupied by material things and some not. But if a certain volume does happen to be wholly occupied by a material thing, then the sense-datum which is identical with the surface of the volume will also happen to be identical with the surface of the material thing. And we can now dispense with Mr. Wisdom's questionable expedients for disposing of dissociated data. We do not have to say that they are

¹ Theories on these lines have been suggested both by Prof. Prichard and by Prof. Broad.

either parts of our brain or parts of the background. We simply say that a dissociated datum is one which is located on the surface of a volume not occupied by any material thing. And we can do more. We can also dispense with the very dubious suggestion that sense-data can appear to have qualities which they do not really have. Further, we can say that an optimal sense-datum is one which exactly coincides with the surface of a thingishly-occupied volume. It is true that we have to pay a certain price for these advantages; we have to accept what has been called a substantialist theory of space. But if we have to choose between Mr. Wisdom's theory and this one, I think that this one is decidedly less paradoxical and more economical.

I shall deal much more briefly with Mr. Wisdom's last two chapters. Chapter X. is on Knowledge of Material Things. Hitherto we have been assuming that there *are* material things, in order to investigate the relations between them and sense-data. We have now to inquire what evidence we have for this assumption. Mr. Wisdom discusses and rejects Phenomenalism, Agnosticism, Spiritualistic Monadism,¹ and Pure Spatialism which asserts that matter has only spatial, temporal and causal properties. The theory which he himself prefers he calls Moderate Materialism. (This is a most unfortunate name, for 'materialism' usually stands for a certain theory about the relation of Mind and Body, and he has himself used the word thus in an earlier part of the book. 'Moderately Agnostic Realism' would be better.)

Mr. Wisdom seeks to justify his choice by various arguments of a causal kind. The fundamental one is as follows.

At intervals during a certain period I sense a number of confamiliar sense-data, *e.g.* a number of 'bookish' ones. Their occurrence cannot be due to the opening of my eyes or the turning of my head (*i.e.*, to the kinæsthetic data thus referred to); for these experiences are often followed by the sensing of quite different sense-data, *e.g.*, 'cattish' or 'treeish' ones. There must therefore be an *external source* of these bookish sense-data. And since the data are, so to speak, always on tap throughout a certain period, no matter at what intervals we open our eyes or for how long, the source probably *persists* throughout that period, even though we sometimes shut our eyes or turn our back or go away.

We can now go farther. By the Principle of Generic Resemblance, the sources must have *spatial properties* of some sort, though of course they need not have the same specific shapes, sizes and locations as the sense-data have. By the same principle, the sources must have *extensible qualities* of some sort or other, which are spread out over them, as colour, hardness, hotness, and the like are spread out over

¹ Mr. Wisdom himself calls it *Mentalism*. But this word, I think, is commonly equivalent to Epistemological Idealism; and Mr. Wisdom, as he says himself, means Ontological Idealism.

sense-data. The source of a coloured sense-datum need not, of course, have the same specific colour as the sense-datum. Indeed it need not have colour at all. But unless it had *some* extensible quality it could not be the source of sense-data which have such qualities. And I suppose it must have *many* extensible qualities at the same time, one corresponding to colour, another to hardness, another to hotness, and so on.

With regard to this chapter, I will only say that if our beliefs as to the existence and detailed nature of material objects can be justified by causal arguments at all, it is difficult to see how the justifying could be better done. (Probably something like Prof. Broad's doctrine of the kinæsthetic Movement Continuum is needed to complete the theory. But as Mr. Wisdom is only giving a brief sketch he may be excused for not discussing this.) However, the argument does assume the validity of Mr. Wisdom's views about Causality, and in particular of his three Causal Principles. And for my own part, I am much more certain of the existence of material objects than of the truth of these principles, especially of the Principle of Generic Resemblance. There is also the difficulty which Prof. Broad has raised against his own very similar theory; *viz.*, that the primary argument establishing the existence of external sources is an argument in inverse probability, and therefore requires that the hypothesis 'there are external sources' should have a finite *antecedent* probability to start with. Perhaps Mr. Wisdom holds that his Principles of Generic Resemblance and Continuity will enable him to avoid this difficulty; on the ground that they, being self-evident, will provide the required antecedent probability. But if they are after all *not* self-evident, nor demonstrable from self-evident premises, the difficulty remains unsolved. It therefore seems to me very desirable to justify our beliefs concerning the existence and properties of matter in some non-causal way if we can. Probably we cannot do so without using *induction* somewhere. How else are we to justify our belief in the continued existence of material objects during intervals of non-sensation? But fortunately not all induction is causal, though all causal laws are inductive.

The last chapter of Part II, like the last chapter of Part I, is somewhat detached from the rest. Its title is *Judgement and Truth*. It is concerned with the relations between acts of judging, propositions, and facts. After a brief criticism of the Pragmatist and the Coherence analyses of truth, Mr. Wisdom adopts the Accordance (or Correspondence) Theory. This naturally leads to a discussion of *propositions*, since it is usually held that a proposition is what accords or discords with a fact. Mr. Wisdom confines himself explicitly to a discussion of elementary or complete propositions. He holds that a proposition is neither an objective entity nor an act of judging. He suggests instead that it is a certain sort of *set* of

acts of judging,¹ or 'an abstraction from' such a set. It is a set of acts of judging 'in abstraction from (a) the particular people who make them, and (b) the particular times at which they are made' (p. 199). And when we say 'the proposition A is B is true' we mean 'any judgement (i.e., act of judging) made by *any* body at *any* time to the effect that A is B will accord with a fact' (pp. 200-201).

Now here the words 'to the effect that' raise a difficulty. A judgement *to the effect that* A is B seems to be just a judgement (act of judging) whose *object* or *judicatum* is that A is B: which brings back the proposition again. Mr. Wisdom seems not to be aware of this difficulty. But he does provide a solution for it. This, if I follow him rightly, is on the lines of Mr. Russell's theory of judgement.² Suppose I judge that this is black now. Then this, the blackness, and the nowness are real elements in the world. When I judge, a peculiar relation comes into being between myself, my act of judging, and these three real entities. Now it may happen that these same three entities (this, blackness, now) are related in the world in the same way as they are related in the judgement. In that case, the situation is as follows: 'there is some fact such that (i) the elements in the fact are identical with the objective constituents in my judgement, (ii) the order of the elements in the judgement *reflects* the order of the elements in the fact' (p. 202, italics in text). In that case my judgement *accords* with a fact. If either of these conditions is unfulfilled, it does not.

But what is meant here by 'reflect'? Mr. Wisdom gives a rather surprising answer. We number the elements in the judgement, and we number the elements in the fact. Then if any element in the judgement which occupies place no. *n* in the judgement occupies place no. *n-2* in the fact, and if this is the case with all the elements in the judgement, the order of the judgement *reflects* the order in the fact. For instance:—

This₁ is black₂ now₃.
I₁ judge₂ that this₃ is black₄ now₅.

Mr. Wisdom adds, astonishingly enough, 'it must be understood that the elements in a fact are numbered from left to right' (p. 203)—and, I suppose, the elements in a judgement must also be numbered from left to right.

What are we to make of this very puzzling remark? No doubt Mr. Wisdom is here assuming the truth of his view, discussed above, that facts are infinitely thin slices of events. But even then it seems so glaringly obvious that in most facts there is no distinction of right and left. How for instance are the elements of *this is black*

¹ Mr. Wisdom himself says *judgements*, not acts of judging. I think this is a pity, since as he points out himself 'judgement' is often used to mean what is judged; in which case 'judgement' would be *synonymous* with 'proposition'.

² *Problems of Philosophy*, ch. 12.

now or this sound is louder than that or I am now puzzled to be distinguished as right and left? Again, how can we distinguish a right and left in a judgement? If 'judgement' here was equivalent to 'sentence' we might do it, provided that the sentence is written, not spoken, and written in English, not Chinese. But Mr. Wisdom makes it quite clear on p. 198 that by 'judgement' he means the psychical act of judging, not the sentence which expresses it. And this act, or rather the many-term relation involved in it, is clearly nothing spatial.

Mr. Wisdom is obviously thinking of the case where a fact contains an *asymmetrical relation*. If the cat is drinking the milk, then it is false to judge that the milk is drinking the cat, though the judgement contains exactly the same elements as the fact does. The point he wants to make can be stated more clearly, though less picturesquely, as follows: If a judgement is to reflect a fact, two conditions must be fulfilled: (1) The judgement must contain the same elements as the fact, and in addition it must contain the judging mind and the judgement-relation; and further (2) if the fact contains an asymmetrical relation R between a and b , and if this relation runs from a to b but not from b to a , then in the judgement also R must run from a to b but not from b to a . Thus in our instance the relation of drinking must run from the cat to the milk, if the judgement is to reflect the fact, not from the milk to the cat.

There are two other points to be noted. First Mr. Wisdom makes it quite clear that he is only concerning himself with the analysis of elementary or complete propositions, such as 'this is red now', and their accordance or discordance with complete facts. If we considered general or incomplete propositions such as 'something is red somewhere' or 'anything which is red in extended' a number of other questions would arise, which he does not here claim to deal with. It may for instance be thought that such propositions do not accord or discord with facts in the same straightforward way. Secondly, it is clear that by 'judgement' Mr. Wisdom means belief or opinion, not knowledge. Knowledge is *of* facts, but it cannot intelligibly be said to accord with them, for it cannot intelligibly be said to be *true*. But unfortunately a large number of people, as Cook Wilson pointed out, have used the word 'judgement' to cover knowledge as well as opinion and belief. Now it is possible that some or even most of Mr. Wisdom's readers may use the word 'judgement' in this wide and to my mind confusing sense; and so may think that he is advocating a correspondence or accordance theory of *knowing*, which he certainly does not intend to do. It would therefore have been better if he had substituted the word 'belief' for the word 'judgement' throughout his argument.

I do not propose to comment on the two brief appendices (on 'Mental and Material Facts' and 'Universals and Particulars'); and there are a number of other interesting points in earlier chapters which I have omitted or barely mentioned. In particular, I have

merely referred in passing to the chapter on Free Will, which to some readers may seem the most exciting in the book. But I hope that enough has been said to show that Mr. Wisdom has produced not merely an excellent introduction to 'Philosophical Analysis as practised at Cambridge' (to quote the dust-cover) but also a number of interesting original contributions to that subject.

H. H. PRICE.

Descartes: An Examination of Some Features of his Metaphysics and Method. By W. A. MERRYLEES. Melbourne University Press (in association with Oxford University Press), 1934. Pp. xxviii + 330. 12s. 6d.

THIS book deals with most of the important questions raised by the philosophy of Descartes. The author's aim, as he tells us in the preface, is 'not so much to give an exposition of Descartes' philosophy, as to arrive at the true answers to the questions he raises'; consequently, he treats Descartes' arguments on their own merits, as if they had been written at the present day, without attempting to interpret them in the light of his historical position. The main topics dealt with are the nature of the certainty expressed by the *Cogito*, the relation of this certainty to the general criterion of clearness and distinctness as a test of truth, the question what this certainty includes and what it does not include, the question whether Descartes commits a *petitio principii* in his arguments for the existence of God, these arguments themselves and their logical implications, the Cartesian view of ideas and the distinction between ideas and judgments, and finally the Cartesian method of clear and distinct ideas as involving the analysis of the complex into its simple components. A full and clear summary at the beginning of the book helps the reader to keep the thread of an argument which at times becomes rather involved. Though on many of these points I cannot agree with Mr. Merrylees, I find his treatment of them in a high degree stimulating and enlightening. He has evidently made a thorough study of the text of Descartes, and he shows himself throughout an acute critic and a close thinker. The book is one which no student of Descartes or of the problems raised by Descartes ought to neglect. The closeness, ingenuity and detailed thoroughness of the argument make it a book hard to treat adequately in a reasonably short space. To be content with generalities would be of little value and unfair to the book, while to deal with the whole of it in the detail which it demands would require a very long review. I shall therefore select for special discussion the first three chapters, which deal with doubt and certainty and with the *Cogito*, and some parts of the fourth, which deals with the epistemological significance of the proofs of God's existence.

In his first chapter Mr. Merrylees draws certain distinctions between the different senses in which the words 'doubt' and 'certainty' can be used. There is the distinction between just feeling certain ('psychological' certainty) and believing that one has adequate grounds for one's certainty ('logical' certainty). The commonsense belief in the existence of the external world is an instance of psychological certainty. Each of these kinds of certainty is such that it may or may not in fact be objectively justified (*i.e.*, the beliefs may in fact be true or false). Mr. Merrylees also draws what purports to be a different distinction—different in both its terms—between being 'subjectively certain' and being in fact justified in your subjective certainty (being 'objectively certain') (p. 7). He distinguishes between psychological and subjective certainty on the ground that psychological certainty may or may not be justified. If not justified, it is 'merely subjective'; if justified, it is objective. This would seem to imply, if the distinction is to be maintained, that subjective certainty, unlike psychological certainty, can never be justified; but Mr. Merrylees certainly does not mean this, for he says plainly that 'objective certainty is simply subjective certainty which is justified or objectively valid', and repeatedly raises the question how subjective certainty may be proved objective. I can see no ground for distinguishing psychological from subjective certainty. No doubt it is not psychological certainty in general, but only unjustified psychological certainty, that is '*merely subjective*'; but '*merely subjective*' is not identical with 'subjective', and surely *is* identical with '*merely psychological*'. The difference between objective certainty and logical certainty is clearer. An objectively certain belief is, by definition, always in fact justified (*i.e.*, true), while a logically certain belief (again by definition) may be based on grounds which, though they are taken to necessitate its truth, do not in fact do so. It does not seem to me legitimate, in dealing with different kinds of certainty, to introduce factual considerations, and we shall find that the distinction between subjective and objective certainty causes trouble later. We must notice next the very important distinction, clearly grasped by Mr. Merrylees, but often neglected by others, between two ways in which subjective certainty may fail to be objectively justified. (He first makes this point, rather obscurely, on pp. 8-9, but he develops it clearly in chap. ii., p. 22, with special reference to the certainty of clear and distinct perception.) We may be relying on a kind of evidence which is in general untrustworthy. In this case our certainty is not justified, even though we make no mistake in applying our evidence—*e.g.*, in applying a general test of truth, such as that of clearness and distinctness. But we may also be liable to error even when the sort of evidence on which we rely is in principle trustworthy. We may err by assuming that this sort of evidence is present with the required degree of completeness, where it is not so present. In this case the remedy is to pursue inquiry further by applying our general test

more thoroughly. Mr. Merrylees expresses this by saying (p. 9) that whenever I am subjectively certain I am justified in saying 'this is objectively certain, provided I am not making a mistake'. Thus I may be right in using the multiplication table and yet may be making mistakes in its use. Mr. Merrylees argues in both passages that the risk of error of this kind is completely eliminated by repetition and by comparison with the conclusions of others working on the same problem. I should maintain that confirmation of this sort can do no more than increase, without limit, an initial probability. Another fundamental distinction drawn by Mr. Merrylees (within the realm of psychological certainty) is that between the certainty of immediate experience, to which neither logical doubt nor logical certainty apply, and other psychological certainties which, though not (according to Descartes, at least) logically certain, are capable of becoming so—e.g., the difference between the certainty that a pain is felt when I am actually feeling it or that *it seems to me* that $2+2=4$, on the one hand, and the certainty that $2+2=4$, on the other. In certainty that has become logical an implication is perceived, and Mr. Merrylees seems to hold that wherever an implication is perceived there is an inference. Hence logical certainty is, according to him, confined to inference. On the other hand, immediate experience gives only matter of fact and not implication. It is a defect in his book that Mr. Merrylees nowhere adequately discusses the mutual relation and interdependence of these two sorts of evidence. Finally, there is the distinction (which Mr. Merrylees seems to regard as of subordinate importance) between unreflective and reflective certainty. There is unreflective certainty 'because the judgment and the evidence on which it is based monopolise our attention to the exclusion of any grounds which there may be for doubting it' (p. 10. I have altered the tense). On the other hand, reflective certainty is only attained after seeking possible grounds for doubt and failing to find them. Mr. Merrylees fails to notice that this distinction is of fundamental importance to Descartes. Before any reference to clear and distinct perception there is a test of truth presupposed in the method of doubt. It is presupposed that a proposition must be accepted as true where reflective search carried to the utmost limits of which we are capable reveals no grounds for doubting it. This suggests the question whether this test is to be taken as coincident with that of clearness and distinctness—a question to which we shall have to return.

The second chapter is on the certainty of the *Cogito*. Mr. Merrylees holds that the *Cogito* formula is intended by Descartes to express an inference. He argues (1) that Descartes only denies that it expresses a syllogistic inference, (2) that the *Cogito* asserts an implication between my thinking and my being, and (3) that therefore it expresses an inference (though not a syllogistic inference). On this view, the existence of the thinking self is only known because it can be inferred from the prior knowledge of something else—namely, its thinking.

I reply to (1) that in the Replies to Objections Descartes was only concerned to deny syllogistic inference, and to (3) that to assert an implication is not the same thing as to make an inference from premises to a conclusion beyond them. Starting from the awareness of a total fact, we may distinguish elements within it having between them an implication which we may proceed to assert, without making an inference. This, I venture to suggest, is what the *Cogito* formula does. It could only be interpreted as an inference if we supposed that what we were primarily certain of was not 'I think', but the existence of (my) thinking, not supposed to be initially known as mine. There would then be the perception that this thinking implies a thinker; and a final step would be necessary to prove that *I*, and not someone else, am the thinker that the thinking implies. This whole position seems quite untenable, nor would Mr. Merrylees, if I understand him rightly, accept it, although in chap. ii., § 5, having (in an attempt at an interpretation eliminating implication) identified 'my being' with '(my) thinking,' he formulates the *Cogito* for this purpose as 'This thinking is' and later adds that from it we cannot advance to the existence of myself as subject 'unless we introduce implication'. But I do not think he means that there is any way of inferring from 'this, or (my) thinking is' to my own existence. At any rate, this position is certainly not Cartesian. For Descartes the primary certainty is 'I think', not 'There is thinking'—I am not primarily certain of any thinking at all except in being certain that *I* think. Now, when I am aware that I think, or that my thinking exists (Mr. Merrylees admits that these mean the same thing, p. 35), what I am aware of is no doubt a complex, between the elements of which there is an implication. The existence of my thinking implies the existence of me as thinker. But this implication is contained within the total fact of which I am primarily certain—the fact that I think or that my thinking exists. 'I think' or 'my thinking exists' is not a distinct premise from which a new fact going beyond it, that 'I exist', is inferred. The *Cogito* is a piece of logical analysis, distinguishing within the 'I think' the thinking and that which thinks, and asserting an implication between them. I urge in support of this view of Descartes' meaning such phrases as 'a primitive act of knowledge', 'a simple act of mental vision', 'as if it were a thing known *per se*', which occur in the passage from Reply to Objections II. (quoted on pp. 17 and 18); but I wish especially to draw attention to a phrase at the end of this passage, where he says that one learns the rule that everything that thinks exists, 'because one experiences, in one's own case (apud se experitur, sent en lui-même) that one cannot think unless one exists'. This is repeated in very similar language (in me experior ut *cogito, ergo sum*) in a reply given to and recorded by Burman (A. and T., v., 147). The use of the verbs 'experiri' and 'sentir' is significant. My thinking and my being are included inseparably within a single experience—are felt together. No demon 'can ever bring it about that I am nothing, so long as I shall think that

I am something' (Med. II.). It is this single experience of self-awareness that the *Cogito* expresses and analyses.

If the *Cogito* were merely the clear and distinct perception that in order to think I must exist, it would prove (as Mr. Merrylees argues, chap. iii., § 3) only that 'if I think, then I am', not 'I am'. Mr. Merrylees asks, therefore, 'Does the *Cogito* prove that I actually do think?' and replies that on the contrary the *Cogito* presupposes and starts from the psychological indubitability of my immediate experience that I think (cf. pp. 32 and 37). If this means that it is absurd to ask for a logical proof that I think, then I agree. It neither needs one nor admits of one. But why say that the *Cogito* presupposes or takes for granted 'I think' (that 'I think' presupposes 'I think'), rather than that this is part of what it asserts? Mr. Merrylees is (I think) debarred from saying this because the *Cogito* is a clear and distinct perception, and as such can only (on his view) assert implication; it cannot express the certainty of an immediate experience. Yet Descartes says, 'I experience (or feel) in my own case the impossibility of thinking without being'; and I shall maintain later that the term 'clear' in 'clear and distinct' refers always to the immediate presence to the mind of that which is perceived.

Mr. Merrylees lays great stress on the doctrine that a general test of certain truth may be derived from the certainty of the *Cogito* (chap. ii., §§ 1-2). He admits, indeed, that Descartes makes the suggestion only in a tentative way, so that it may be only a stage in the course of his meditations and not a result finally accepted. But he holds that the Cartesian argument at this point is completely valid and that Descartes must have regarded it as completely valid. Now, it must be admitted that if a certain kind and degree of evidence places the *Cogito* beyond the reach of doubt, the same kind and degree of evidence, wherever it is found, will place other truths beyond the reach of doubt. If we call this evidence 'clear and distinct perception' it follows that whatever we perceive with the same complete clearness and distinctness with which we perceive the existence of the thinking self must be true. But this leaves unanswered the question whether there are any other truths perceived in this way and, if so, what these are. Descartes proceeds to show that what ordinarily passes as clear and distinct perception of truth (e.g., in Mathematics) may not be so. Granting that all other openings for doubt are excluded, there still remains one ground for doubting which, however far-fetched, we must not neglect. Ordinarily we assume that the source of our being is such that it does not positively and systematically deceive us. But we do not start with any clear and distinct perception of the nature of the conditions to which we owe our existence. The author of our being may be a malignant demon. The doubt aroused by this possibility can only be dispelled by the proof of the existence of a veracious God. In what precise way does Descartes suppose that such a demon might deceive us? Mr. Merrylees rightly argues (pp. 59 ff.) against the prevalent view that it is to guarantee the accuracy of our memory

that Descartes appeals to the divine veracity, though he maintains that the argument in Reply to Objections IV. is exceptional, and admits of no other interpretation. According to him, all the other relevant passages, though differing in the line they draw between the perceptions that do and those that do not need the divine guarantee, are agreed in maintaining that the guarantee is required 'to assure us that the subjective certainty we entertain when we perceive something clearly and distinctly is valid' (p. 67). It seems to me that Mr. Merrylees has come very near to giving the right solution of this tricky problem (*cf.* especially pp. 64-5), though I cannot follow him either in the use he makes of the sharp distinction between subjective certainty and objective validity, or in the special view he takes of Reply to Objections IV., or in the subtle differences he discovers in the other passages about the application of the guarantee (as distinct from what it guarantees). I can best bring out what seem to me the strength and deficiencies of Mr. Merrylees' position by first stating my own, which is, I think, closely akin to it. The general test of clearness and distinctness as derived from the *Cogito* is itself beyond the reach of the most malignant and powerful demon (as the *Cogito* itself is—for being deceived is thinking). But this does not prevent such a being from deceiving us. All that he need do is to make us believe that we have a clear and distinct perception of truth where we have not, and to do this in such a way that no further investigation can possibly reveal the error to us. To apply Mr. Merrylees' language, we should then be objectively certain, unless we were making a mistake.¹ Now, it is possible that the author of our being has in fact so constituted us that we do make a mistake of this kind which we are powerless to correct. It is true that this assumption is very far-fetched (Descartes calls it 'hyperbolic') and cannot suggest itself to common sense or science. It can only emerge when the method of doubt is pushed to its extreme limits, as

¹ This seems to me a legitimate application of his distinction, in spite of his argument on p. 21 that we cannot contend that 'where [I should prefer 'if'] we make a mistake, we do not perceive clearly and distinctly, but only think we do'. This seems so plainly inconsistent with other passages that I feel I must have missed the point. Even the alternative solution offered on the next page seems simply a reassertion of this one that has been ruled out. "It [what we perceive clearly and distinctly] might be false because we are making a mistake; because our perception is defective; because the implication we believe we see does not in fact hold" (p. 22). (Does the first passage, perhaps, apply only to clear and distinct perceptions *while we are experiencing them*, and the second only to retrospective doubt?) In the passage under discussion, Mr. Merrylees rejects the contention because its acceptance would involve complete scepticism. The reply is that (1) apart from the demon hypothesis, Descartes never admits even the *possibility* that perception that is really clear and distinct may be mistaken, and would certainly not accept Mr. Merrylees' 'fact' that we sometimes make mistakes in regard to what we perceive clearly and distinctly (p. 21); and (2) the retrospective *doubt* engendered by the demon hypothesis does not, as we shall see, finally land us in scepticism.

it is in the philosophy of Descartes. Further, it cannot emerge while we are directly contemplating the evidence of the apparently self-evident propositions of mathematics, etc. For then the evidence itself monopolises our attention and compels conviction. It is psychologically impossible to entertain the doubt arising from the malignant demon hypothesis except in retrospect, when we only remember that something has appeared to us so evident as to exclude doubt. But prior to the proof of the existence of God, and if we set aside the *Cogito*, it is always, or at least very often, possible to doubt whether what has appeared to us to be clearly and distinctly perceived really was clearly and distinctly perceived. The doubt is not a doubt of the validity of memory. We remember correctly that we could not help believing that we were perceiving clearly and distinctly, but we now wonder whether perhaps a demon may have tricked us into that belief. The point, then, is that in retrospect the mind is set free to entertain a doubt which it cannot entertain while directly confronted with the evidence. It follows that the criterion of clearness and distinctness remains in principle unchallenged. The doubt only concerns the application of this principle. Hence it is not a *petitio principii* to use it in deciding the question whether or not the source of our being can be such as positively to deceive us and to deprive us of any means of detecting the deceit. There is much more to be said on this question—e.g., that the doubt itself presupposes that we have a cause and therefore presupposes the principle of causality; but I have dealt with this elsewhere [MIND, vol. xxxviii., p. 470, and vol. xlii., p. 371 and p. 373].

I maintain that this interpretation fits all the relevant passages (if we assume that the ambiguities of the First and Third *Meditations* may be interpreted in the light of the re-statement in the Fifth), and further that it shows the appeal to the divine veracity to be both non-circular and epistemologically necessary, as against Mr. Merrylees' view that on any interpretation, including all his own interpretations of inconsistent passages, it is 'either epistemologically superfluous or circular' (p. 71). Mr. Merrylees, in examining the Reply to Objections IV., rejects what is in effect my solution on the ground that 'it means that the original perception was false, and obviously we cannot make a false perception true by proving that God exists' (p. 60). But plainly it merely implies that the original perception is doubtful—doubtful in retrospect only and on the strength of a far-fetched hypothesis, but otherwise beyond doubt. If this is granted, there seems no need to take this passage as maintaining (unlike the rest) that the guarantee applies to memory. Mr. Merrylees (p. 65) puts his view of the divine guarantee (so far as he thinks it possible to formulate a single interpretation covering most of the passages) as follows: it assures us 'that propositions which we remember being certain were true really are true, despite the fact that we are no longer assured of this by the evidence' or (in other words) 'that the certainty was not merely subjective, but was objectively valid'. I should emend to 'in spite of a doubt which could not arise when the

mind was occupied with the evidence'; and I doubt if the distinction between subjective certainty and objective validity (or certainty, as Mr. Merrylees also calls it), adequately brings out the point. All certainty is subjective in the sense that it is a thinking subject that is certain; and all certainty (as Mr. Merrylees himself recognises, p. 8) is objective in the sense that it is the certainty that something is objectively true (all 'subjective certainty' claims to be 'objectively valid'). Our certainty is never for us, while and so far as we are certain, 'merely subjective'; if the question genuinely arises for us whether our 'subjective certainty' is 'objectively valid' (i.e., whether what we believe is true) we have so far ceased to be subjectively certain. The important difference here is surely not between 'subjective' and 'objective' certainty, but between certainty reached through inadequate and certainty reached through adequate evidence. We may be certain although we have not examined all possible alternatives (e.g., from laziness or because we want to believe something) or, on the other hand, we may be certain because after a thorough search we can find no other tenable alternative. Our certainty while we are having a clear and distinct perception (e.g., that $2 + 3 = 5$) is of this latter kind, for so long as we attend to the mathematical evidence there is no room for any alternative. However thoroughly we search, we can find no ground for doubting that we are clearly and distinctly perceiving a truth, and we are therefore justified in our belief. Such a ground can occur to me only when I have ceased to attend to the proposition in question and have faced the possibility that I am 'so constituted by nature that I can easily deceive myself even in those matters which I believe myself to apprehend with the greatest evidence and certainty' (Med. V., my italics). At no stage is the criterion of clearness and distinctness called in question. Nor is the proof of God's existence itself subject to retrospective doubt, for it has removed the only ground for such doubt.

The question remains whether the evidence which makes the *Cogito* certain really is clear and distinct perception, as clearness and distinctness are elsewhere defined by Descartes. In other words, does this evidence depend upon clear and distinct ideas? I gather that Mr. Merrylees would deny this, on the ground that it also rests on immediate experience. Now, Descartes certainly does not ignore the part played by immediate experience. It is essential to his position that ideas only exist at all in being perceived, and in saying this he refers not only to ideas in the proper sense but also to affirming, denying, desiring, etc. In general, every mode of thought exists only if and so far as the thinker is immediately conscious of it. (On this point see Veitch's note 1.) Hence he insists that though the existence of that which an idea purports to represent may be doubtful, this does not make the existence of the idea itself doubtful. On the other hand, he does not hold that immediate experience by itself yields certain judgments. In order to attain complete certainty, even in asserting what we immediately experience, we must have a

clear and distinct apprehension of what it is that we assert. This is well brought out in the passage in the *Principles* (§§ 45-46) where Descartes explains what constitutes clear and distinct perception. He selects for an example what is immediately experienced when someone feels an intense pain. The pain is clearly perceived inasmuch as it is present and manifest to the mind giving attention to it. But judgments about it are not therefore exempt from error. The attentive mind distinguishes it within the complex whole of which it is part, but in doing so it may include along with the pain something which does not properly belong to the nature of pain. Thus it may consider the pain not as purely a 'mode of thought,' but as an occurrence in the body. No complex experience which did not contain pain could be identified with a complex experience which does; but within this complex experience the pain may not be precisely separated from other factors present to the mind. When this is so, Descartes would say that the pain is clearly but not distinctly perceived. It may be said that mere clearness is sufficient to give certainty of the existence of the pain. In a sense this is true, and I believe that Descartes would agree. There is certainty, but there is no precise definition of what it is that is certain. Hence the certainty is not complete. There is room for what I may call collateral error. For complete certainty distinctness as well as clearness is needed. The complex whole of which the pain is part must be analysed into its simple constituents, so that each of these is distinguished from it and from the others. In Cartesian language, our idea must not only be clear itself, but must contain nothing which is not clear. If we turn to Descartes' account of the *Cogito*, we find implicit in it the same distinction between clearness and distinctness. Initially we are certain of our own existence as thinking beings, but further reflexion is needed in order to determine precisely what it is that we are certain of. In other words, the certainty is incomplete inasmuch as it depends on a clear but not on a distinct perception of our nature. To begin with, we are prone to ascribe to the self of which we are certain characters that belong to body. But this confusion is removed when we recognise that the certainty of our own existence remains untouched although we doubt or deny (whether truly or falsely) the existence of anything else, including all that pertains to the nature of body. We thus get not only the clear but also the distinct idea of the self as purely a thinking being. It is on this ground that Descartes asserts that nothing belongs to the essence of the self (of whose existence we are certain) except thinking. He does not, as Mr. Merrylees seems to suppose, gratuitously assume that this is so. It is quite another question how far and in what way the thinking being as such is a substance—i.e., capable of existing independently of other things. Descartes does not even attempt to show that mind is capable of existing apart from body until he comes to the *Sixth Meditation*. I agree with Mr. Merrylees that his argument there is not valid.

There is much more that might be said on this topic of the *Cogito*,

but I have said enough to show in general what Mr. Merrylees' position is and how far I disagree with it. It would be no use dealing with the rest of the book unless I did so in equal detail, but limits of space forbid this. I shall only note in conclusion one point. Mr. Merrylees sets himself to show, at some length (chap. iv., § 8), that Descartes does not need to prove God's veracity in order to establish the existence of the external world. He is able to do this because, in the first place, he does not recognise that for Descartes himself it is his fundamental representationism, not 'the fact that sense-perception is sometimes deceptive' (p. 89), that makes the claim of sense-perception to objective validity doubtful *in principle*, and therefore in need of a guarantee (the knowledge that sense-perception sometimes deceives is not, Mr. Merrylees rightly points out, a valid ground for concluding that *all* sense-perception may deceive, since we cannot know that one sense-perception is 'false' unless we know that another is 'true'); and because, in the second place, he takes the essence of the proof of the existence of matter to lie in the need to explain the existence and more especially the usefulness of the faculties of imagination and sense-perception. If this is granted, it follows that 'the argument [*i.e.*, through the divine veracity] cannot begin, unless we assume that the faculty of perception is not useless', and, therefore, even if it is valid it is superfluous. But Mr. Merrylees ignores the part played in the argument by the 'teaching of nature'—an essential factor which is quite other than clear and distinct perception, and does need the divine guarantee. (For a full treatment of this question I may refer to my article in *MIND*, vol. xli., No. 162.) The utmost that clear and distinct perception can yield is that our sense-experience has some adequate cause, not that it is caused by bodies. Apart from reference to the veracity of God we believe in the real existence of bodies only because we have a strong instinctive propensity to do so (Mr. Merrylees is saying just the same thing when, speaking for himself, not for Descartes, he says (p. 89) that 'perception, of itself and from the beginning, claims objective validity'). What Descartes has to show is that this 'teaching of nature' is guaranteed by the veracity of God. (The question for him is just what Mr. Merrylees, again speaking for himself, takes it to be—'whether the claim of perception to objective validity is in general justified'). He does so by pointing out that God would positively will to deceive us by implanting in our nature an instinctive propensity leading us into error which we can neither avoid nor correct. Other teachings of nature may be, and frequently are, partially erroneous, but a thorough application of the criterion of clearness and distinctness is capable of showing not only that they are erroneous but how and why they are erroneous. But no application of this criterion will correct the general tendency to believe in the existence of bodies. If this instinctive tendency is misleading, the author of our being has, so to speak, told us a positive lie; and this a veracious God cannot do.

A. K. STOUT.

VI.—NEW BOOKS.

The Philosophy and Psychology of Sensation. By CHARLES HARTSHORNE.
Chicago: University of Chicago Press, 1934. Pp. 288. 13s. 6d.

THIS book raises the very important question: What world is it in which we live? the world of sense-data, or the world of physics, or a world, analogous to the world of physics in *not* being the world of sense-data? The last alternative must be added because it is not easy to see how far Prof. Hartshorne's Panpsychism goes.

Common-sense man lives in the coloured, sounding, scented, touchable world of sense-data. His feelings of joy, fear, expectation, pain and hatred are directed towards people, animals, things and flowers, and not towards wave-packets. Cognition precedes affective response, and it is the cognised as cognised that arouses the emotions.

This, too, is the world with which the psychologist starts. If he is interested in the physical changes which cause him to have a perception of a coloured patch or middle C he works backwards, as it were, into the physical world which 'lies behind' phenomena; if he is interested in behaviour, he assumes the world of sense-data and makes out a scheme of instincts, tendencies, drives or what not, to account for our responses. If he is an epiphenomenalist, he talks about the physical world (inside and outside the body) and the phenomenal world as well, often flitting illegitimately from one to another; but one always has the impression that it is the world of common-sense that he really has in his mind as the agent that activates the nervous system, and, indeed, that the nervous system he talks about is what it looks like, rather than what physicists pretend it 'really' is.

All this means that we take the content of our awareness as ultimate. Certain wave-lengths are seen as red because they are, and that is the end of the matter. And it is this irritating ultimacy, this complacent acceptance of middle C as inexplicable, that exasperates Prof. Hartshorne.

He tries to penetrate the veil of sense-data and seeks the fundamental response of Life to an environment. "The basis of experience is emotional", says Dr. Whitehead, and Prof. Hartshorne agrees, adding "and social", though he uses that adjective in rather a peculiar way. "Let us mean", he writes (p. 193), "by social feeling merely 'feeling of feeling'." The 'primary meaning' of 'over-there-ness' is a social form of response.

In a chapter on 'The Dimensions of Experience' he analyses the axes of measurement along which Life's affective responses are made. Firstly there is the self-other axis with various degrees, some affective responses being more, others less, intimate. Secondly there is the active-passive line. Thirdly there is the positive-negative polarity of which "pleasure and pain, joy and sorrow, love and hate are expressions" (p. 195). Lastly there are variations along the axis of intensity.

And now we turn again to the question: In which world are these emotional responses felt, or, rather to which world are they responses?

Let us quote Dr. Whitehead's 'Adventures of Ideas' once more. "The true doctrine of sense-perception is that the qualitative characters of affective tones inherent in the bodily functionings are transmuted into the characters of regions."

This is the thesis of Prof. Hartshorne's book.

Life affectively responds, or, perhaps, *is* a trembling affective continuum. The sense-data which seem so independent, which make up our world, are our affective responses to a non-presentational environment. From our social natures is derived the spatial frame-work. "The 'distance' of blue, the 'aggressiveness' of red embody modes of variation fully explicable only in terms of experience conceived as a social continuum" (p. 8). "The mere fact", he writes on page 196, "that nearness means greater fullness of detail proves that the near is the more intimate and vivid". By means of a telescope one can be on friendlier terms with the moon. Again, and more surprisingly, he says: "Thus a triangle is the only mode of social grouping in which three individuals may share alike in the same relationship to the group as a whole." Geometric figures are derived from sociality and so our 'three-cornered affairs' are truly so-called.

Since Life has its way to make in the world, the fundamental shape of its responses are adaptive in nature, so that, apart from the social element, the world as we see it is a function of intensity and success of response and their opposites. But the writer goes further than that. "Of all the colours blue is intrinsically the most appropriate to adapt the human organism to the principal objects in nature (the sky, distant objects, and bodies of water) which reflect the short wave-lengths perceived as blue." This is supposed to be quite obvious, and it is thus that Prof. Hartshorne hopes to improve the position of psychology as a science, by 'explaining' the blueness of blue. But even if we grant that intensity and viability are the keys to the situation, we cannot *infer* 'blue' from any given intensity and viability. The quality of the sensation will always be a matter of fact and not a matter of necessity. And, furthermore, the more Prof. Hartshorne reduces his ultimates, the more 'inexplicable' are the actual presented varieties.

This explanatory story bulks large in the book, but it is not the most interesting part of it.

Prof. Hartshorne not only claims to explain why wave-frequencies of 256 per second make us hear middle C, but he believes he has reduced our ultimates in another direction as well.

It follows from his theory that variations of sensation are really variations in affective response. This means that there is a closer relation between different kinds of sensation than we have hitherto believed to be the case, because the ways in which affective responses may vary are fewer than the accepted variety of sensations. Along the axes of experiential dimension we shall find sounds, colours, tastes and smells cheek by jowl.

The importance of this lies, not so much in the fact that it follows from his general thesis, as in the fact that it concentrates our attention seriously on the affective element in sensations, and on the interconnections between sensations, both of which notions find support in popular and poetic speech. The 'growling thunder', the 'aggressive red', the 'gay yellow' and the 'sweet sound' are mentioned. The Americans 'feel blue', a feeling-tone may be 'bitter-sweet', and the vulgar speak of a room 'smelling cold'. Readers of modern poetry will doubtless think of suitable quotations.

Prof. Hartshorne insists, to my mind rightly, that these 'associations' are not based on association, but are more intimately connected with the sensations themselves. He goes so far as to say: "The 'gaiety' of the yellow (the peculiar highly specific gaiety) of the yellowness of the yellow."

This may seem to be going too far. Prof. Hartshorne uses the adjective 'logical' a great deal, and it might be asked at this point whether he really thinks that the proposition: 'yellow is a gay colour' is a tautology.

But, for all that, his evidence is important. The sound of a military band does seem to stir many people 'to the depths' as well as giving them sensations, and the experimental work of Nafe, Pickler and others, referred to in this book, supports the theory of immediate emotional disturbance, alongside of sensation, which seems too objective to be an acquired meaning.

Prof. Hartshorne seems to think that if the emotional quality is localised where the sensory quality is localised, they must be identical, but his argument does not seem to me to be conclusive.

Another allied point of interest is the light thrown by a theory, which aligns sensation and affectivity, on the difference between seeing and hearing on the one hand and smelling and tasting on the other. It cannot be denied that the latter seem to be more of the nature of subjective modifications than the former. Then, again, there is the painful sensation of pain. Pain is alleged to be a sensation, but is its painfulness really distinct from it, associated with it, or is it identical with it?

Such are the considerations which Prof. Hartshorne brings before us, with a wealth of detailed treatment of colour vision, pitch, and the quality of brightness which is predicated of both.

We are still left, however, in difficulties. If affectivity is the fundamental fact in living response, for which there is much to be said, and if the world of common-sense is really a world of projected affects, what about the 'secondary' emotions, which seem so 'vital' to us in every-day life? The mourner and the bean-feaster may both pass a field of gay yellow buttercups. True, the gaiety may jar on the mourner and be welcomed by his fellow, bent on pleasure, but ought they not to see different colours? Can we believe that the man who goes in fear of danger is not fundamentally shaken? Surely Life is responding through him, or in him, in such a way that one might expect that his world would be materially changed in virtue of his emotional response, if it be the disturbance of Life that gives us the material world.

Prof. Hartshorne hints at a Spinozistic theory to the effect that we really feel the feelings of our cells. "If anything is directly known to be qualified by redness, it is one's body when seeing red" (p. 246). But the ownership of the body seems to be the society of all the cells 'in total sympathetic connectedness', which makes the doctrine of 'knowing one's body' a difficult one to maintain. Peirce is quoted as saying: "Consciousness is a sort of public spirit among the nerve cells," but even if one takes up this curious position it is not easy to see how one can interpret the emotions which we have towards sense-data as being the emotions of the whole society of cells towards the emotions of a few.

If we apply this view to the problems mentioned above we should, I think, have to say that some of the cells were feeling gay while the society as a whole was feeling sad, and when the terrified man watches the slinking yellow tiger, it is towards the gaiety of some cells that the society of all the cells feels fear. "Black," he says, "is the very negation of life"

(p. 58). We may therefore contemplate with astonishment the pleasure aroused when we perceive the *néant* of a pickled walnut.

At first one is struck by the paradoxical nature of Prof. Hartshorne's theory, but one cannot help being shaken by his evidence and many of his arguments. Even if his views are open to question, there is no doubt about the importance of his subject. He has attempted to work out carefully some of the implications of the 'Philosophy of Organism', he has called our attention to certain very puzzling connections between affect and sensation, and between sensations of one mode and sensations of another, and we must be grateful to him for his contribution.

W. J. H. SPROTT.

The Frontiers of Psychology. By WILLIAM McDUGALL, F.R.S. Cambridge University Press; London, Nisbet & Co., 1934. Pp. xiii, 235. 5s.

A SPECIAL science can be constituted in two ways, either by selecting as its subject matter a particular set of problems, or by devising a special standpoint from which to handle a subject which may be treated also in other ways. In either case interscientific disputes may arise. A science may encroach both on the problems and on the method of another science. Interscientific disputes, however, are damaging to the *prestige* of the sciences which indulge in them, and may be humiliating to the *amour propre* of the professors who are defeated in controversy. So the prudent professor prefers to fight shy of questions which may lead to an interscientific dispute. He can easily avoid such encounters by confining himself to the most unquestioned and technical part of his own subject and cultivating his own garden, impenetrably hedged round by the barbed wire of a technical language which no one else ventures to attack, because no one else feels sure he understands it. In consequence of this policy a map of the sciences will show at the centre of each science a strongly held castle or citadel, garrisoned by highly specialised experts, who are concerned mainly about their own dignity and safety, but do not care to cultivate relations with their neighbours. Beyond the radius of their influence there extend large areas, disputed and weakly held, practically a no-man's-land, in which vagrant philosophers, pseudo-scientists, and other raiders are left free to operate. Thus in general the frontiers of the special sciences will be ill-defined.

This is the situation which Prof. McDougall has discerned, and which he courageously sets himself to remedy in the case of his own science, psychology. Certainly its situation is sufficiently chaotic and distressing. Psychology can lay claim to an enormous territory, but is in effective occupation of very little of it. Its garrison, moreover, is divided into a large number of discordant bands, much more anxious to levy war on each other than to make good the claims of psychology as a whole, and its frontiers are nowhere delimited properly. Yet they ought to be, for there is a psychological side to every scientific research, and hence a frontier line to be drawn; a proper division of frontiers between psychology and the other sciences should therefore be of the greatest benefit to all.

In his first chapter Prof. McDougall begins by assailing the mechanistic assumptions of science, and particularly the taboo on teleological explanation. He points out (p. 15) that science aims at intervening in the course

of events, and controlling them in accordance with our desires and needs; psychologically speaking, therefore, it is *founded upon teleology*. He rejects, therefore, not only the old 'billiard-ball' mechanism, but also the neo-mechanism of Needham, which defends the mechanical tradition by pleading that, though fictitious, it works pragmatically, and (incidentally), by appealing to a psychology of 'instincts' and 'mental twists'. This McDougall regards as a gross misuse of the Pragmatic Principle, and therefore devotes his next chapter to it.

I regret to say, however, that though he is certainly right in holding that every science is fundamentally pragmatic *qua* purposive, he gives a very queer account of the Pragmatic Principle itself. He does not take the precaution of insisting that 'propositions' are unmeaning formulas, except in connection with some use to which they are to be put. So he illustrates scientific truth by "this flower has five petals," and proceeds "you count the petals, you find five. Your expectation is satisfied, and you say you have verified the proposition; the description is true because it *corresponds* to the reality, the objective fact" (p. 25).

But why, I ask, should the 'correspondence with reality' be regarded as more essential than the 'satisfaction of expectation'? Presumably the petals would not have been counted unless some question had arisen about them, and some one had interested himself in the problem of how many there were. What, moreover, if observation had more or less *disappointed* expectation? It would not invariably have been certain that the 'proposition' was false. There might have been a miscount, or a fusion of two petals, or the consumption of one by a caterpillar.

So Prof. McDougall's inference, that the 'correspondence' theory of truth is *the* valid theory, does not follow. It does not "state the only intelligible and tenable meaning of the word true" (p. 26). Nor does pragmatism "accept the correspondence theory of truth" (p. 27). It regards it as tricky, honeycombed with ambiguity, and usually unmeaning. It does not think that 'reality' (or 'fact') as it appears at the beginning of a scientific inquiry, can be made the criterion of truth, simply because the inquiry so often reveals that it was merely a *reality-claim*, which the inquiry failed to substantiate, or modified profoundly.

Nor can I think that McDougall's account of historical truth (p. 26; expanded, but not improved, pp. 34-36) is at all tenable. It makes truth wholly hypothetical and unverifiable, and ignores the full analyses given of this case by other pragmatists. On the other hand he scores (p. 30) over the physicists in pointing out that Heisenberg's principle implies that observation is always active intervention, and that they erred in thinking that they need make no allowance for the observer.

In the end Prof. McDougall is moved to recognise "two distinct meanings of the word 'true', as applied to propositions, judgments, and beliefs" (p. 38), and as applied to values: he suggests 'valid' or 'sound' as the proper adjectives for the latter. He proposes, further (p. 39), that "the pragmatic principle applies only to the former . . . and does not apply in the realm of value or opinion". Here I would point out that no provision is made for the conception of the 'valuable' (though not 'valid'), and that it is disastrous to give up the pragmatist endeavour to unify the realm of values.

In his third chapter McDougall discusses the relations of science and philosophy, and tries to delimit their fields by assigning to science "knowledge of fact, of propositions which can be brought to the pragmatic test and used as guides to action" (p. 46), while to philosophy belongs "the

province of wisdom, of values and valuation, of valid principles and sound opinions". This division of territories plainly will not do, simply because 'facts' invariably turn out to be values, so soon as we ask, how did they come to be recognised as facts, and to triumph over all that has now lost the status of 'fact'? The only conceivable answer is 'by developing superior value,' and this can always be shown historically.

Nor can I see how, on his own principles, Prof. McDougall can say "the realm of values belongs indisputably to the philosophers; it is and must forever remain their proper and their sole field" (p. 51). For are not values and valuations psychic facts, and so within the domain of psychology? It is doubtless true in a way that "the pragmatic principle subordinates truth to value" (p. 47); but is not the reason that it recognises 'truth' as a value? Thus it does not "put science under the rule of philosophy" in any invidious way (*ibid.*). The pragmatic method is the scientific method *par excellence*, and is used in all knowing; but the province of philosophy is distinct from that of the sciences because the latter do not cover the whole ground of knowledge. For the sciences are all *special* sciences, and rest on a great variety of abstractions. Also they all make one great abstraction, *viz.* that from personality and the personal differences between the *data* as they appear to each inquirer. One has only therefore to declare that philosophy does *not* abstract from personality and personal differences between the *data*, to get a sharp and perfectly defensible frontier between science and philosophy, and moreover one that should be entirely congenial to the spirit of McDougall's activist psychology.

Having thus cleared away preliminaries, Prof. McDougall proceeds in ch. iv. to discuss the relations of psychology to the other sciences, and to show that it is concerned equally with the sciences of mind and with those of matter. Further discussions follow of the psychological frontier toward the mathematical and physical sciences (ch. v.), toward Space and Time (ch. vi.), toward Energy and Force (ch. vii.), Perceiving and Conceiving (ch. viii.), Force and Energy in Nature (ch. ix.). Ch. x. discusses whether Energy is an independent existent, ch. xi. the physicists' attitude towards values, ch. xii. considers the frontier between History and Psychology, ch. xiii. the Problem of Human Progress, ch. xiv. Human Progress as Evolution, ch. xv. its Fundamental Cause, and ch. xvi. the Nature of Value as a Frontier Problem. All these discussions are so full of important and valuable remarks that the reader hardly notices that one most important frontier line is left undrawn. Prof. McDougall gives no indication as to how he would draw the line between psychology and logic, between the ways men *do* proceed in thinking and the ways they are told they *ought* to proceed. In most logicians and psychologists this omission would unhesitatingly be set down to *fear*, due to the difficulties of the problem: but Prof. McDougall has shown far too much courage in challenging traditions to be suspected of any craven motive. It remains regrettable, however, that most logicians should continue to prescribe canons to thought without regard to the natural procedures of human thinking, and that most psychologists should describe the procedures of human thinking without reference to the purposes to which they are intended to be put. In spite, however, of these omissions and the defects apparent in his account of the pragmatic method (to which one might add that the vague remarks on p. 53 about the pragmatism of William James and of the present writer should not have been made with no attempt at substantiation), one can hardly take leave of Prof. McDougall's

book without confessing that it is based on an original and fertile idea, and is worked out in an instructive manner.

F. C. S. SCHILLER.

L'Année Psychologique: Trente-deuxième Année (1931). } Two vols.
Publiée par H. PIÉRON. Paris: F. Alcan, 1932. Pp. xx + 949.
120 fr.

THIS number of *L'Année*, in addition to the usual abstracts and bibliography, contains seven original papers and two shorter notes. Of the papers, that by Piéron on "The Latency and Establishment of Colour Sensations" is possibly the most important, though on the whole this number is less interesting than we generally expect *L'Année* to be. Piéron's paper describes a continuation of work an account of which was given in *L'Année Psychologique*, vol. xxix. In the present series of investigations an attempt is made to eliminate the influence of the variations in the brightness value of colour sensations by maintaining a constant standard of brightness. The main facts now ascertained are: (1) that there is a minimal duration of excitation necessary to give a colour sensation, which is greater the lower the degree of brightness and the less the purity of the stimulating flux, (2) that the establishment of the colour with increase in the perceived saturation is more rapid in proportion as the degree of brightness is high, but its rapidity is not influenced by the purity of the stimulating flux, (3) that in conditions of identical brightness the establishment of red is slightly more rapid than that of blue, and that of green is intermediate, (4) that the difference between red and blue in action time necessary to obtain a saturation equal to that of a lasting stimulation corresponds to the difference in reaction time, and is about three-hundredths of a second, and (5) that this difference is in accord with the explanation previously given of the Fechner-Benham colours.

M. Foucault in his paper on "Mental Work without Movements" replies to critics of his previous investigations by describing experiments with Kraepelin's *Rechenhefte*, in which he employed somewhat complex control methods devised to meet the various criticisms. These control methods which aimed, on the one hand, at eliminating the influence of the muscular work involved in writing down the results of the additions, and, on the other hand, at eliminating the effect of differences in difficulty between the different columns, seem to be satisfactory and adequate. The chief interest of the paper lies in the descriptions of the control methods adopted.

A paper by A. Fessard on "Neural Rhythms and Oscillations of Relaxation" is a long review of recent work in neuro-physiology, more particularly the work of Adrian and his co-workers. The results of this work, as the author remarks, are of interest to psychologist and physiologist alike. Unfortunately, the work has not yet proceeded so far as to realize very many of the hopes which the psychologist justifiably entertains. That these results are of interest and importance to the physiologist goes without saying.

The number also contains a short paper by F. L. Ruch on "The Appreciation of Time by the White Rat", a paper by J. M. Lahy and S. Korngold on "The Selection of Calculating Machine Operators", one by Durup on "The Complexity of Visual Impressions of Movement", and a continuation of Chweitzer's account of his experimental study of the learning curve.

J. DREVER.

L'Année Psychologique : Trente-troisième Année (1932). Two vols. Publiée par HENRI PIÉRON. Paris : F. Alcan, 1933. Pp. xx. + 940, 120 fr.

THIS number of *L'Année* consists as usual of a number of articles, several shorter notes, and abstracts of the literature in all branches of psychology for the preceding year. The original articles are : "The Sensorial Basis of Knowledge" by Piéron, "The Course of Fixation of a Series of Words" by Foucault, "Classification of Pupils by the Teacher, by Tests and by Chance. Comparison of the Results" by P. Quercy, "Investigations with a view to the Interpretation of the Purkinje Phenomenon" by Durup and Piéron, "The Laws of Practice in Voluntary Movements" by H. Gavini, "Hypnagogic Phenomena and Invention" by Durup, "The Analysis of Reaction Time" by Piéron. The three shorter notes are on : "The Paradox of the Unfavourable Action of Blue Light on Visual Acuity" by Piéron, "Auditory Acuity among School Children" by Weinberg and Fishgold, and "Re-learning and the Transfer of Training" by Chweitzer.

The number of highly technical papers is rather larger than usual, but all are interesting. The shorter notes may be taken first. Chweitzer has obtained data which appear to show transfer of training very clearly, so clearly that he expresses the intention of pursuing the investigation. A subject in a learning experiment carried out some years ago showed the usual learning curve. Three years afterwards, in another practice experiment, employing the same material, this subject gave a new learning curve, somewhat flatter but of the same general type as previously, starting, however, at a point very little below the point at which the previous practice had finished. The subject was then given a period of practice with a different kind of material, and showed quite different phenomena. The curve started practically where the other left off, and was relatively flat.

The note on the acuity of hearing among school children is virtually an article, both as regards length and as regards importance. The authors, employing the Western Electric Company's Audiometer, Model 5A, determined the threshold of auditory acuity of some four or five hundred children, ranging in age from six to sixteen. The results showed : (a) progressive though slight increase of acuity with age up to ten, (b) thresholds slightly above American norms, (c) approximately normal distribution of thresholds, (d) superiority of the left ear in the majority of cases, and (e) 'ascending' threshold as a rule better than 'descending' threshold.

The note on the unfavourable effect of blue light on visual acuity is also interesting. Theoretically, Piéron says, visual acuity should be better in monochromatic, and particularly blue, light, than it is in white light. In white light there is always chromatic aberration, which by producing diffusion circles should diminish the resolving power of the visual apparatus. Moreover, the resolving power of an optical system is greater the smaller the wave-length of the light employed. Yet experiment shows that with apparently equal brightness visual acuity is much less in blue than in white light, or in red monochromatic light, which has a much greater wave-length. The explanations that have usually been given—the illusion of apparent brightness in red as against blue light, and the influence of contrast which is less in blue than in red light—would not account for the striking differences we actually find. Piéron suggests that there is in addition for the normal emmetropic eye definite myopia for blue light, and

that this is the main cause of the phenomenon. This suggestion is confirmed by an experiment with myopic glasses on a normal eye.

The paper by Durup and Piéron on the Purkinje phenomenon is somewhat closely connected with this note. The authors indicate that their investigation is more or less preliminary. Nevertheless, the present paper must be regarded as an important contribution to the physiology and psychology of the senses. The paper begins with a statement of the nature of the problem. The Purkinje phenomenon has been interpreted in two very different ways. As a result of the discovery of the duplicity of the retinal receptors—the cones and the rods—the predominance of one or other of the two parts has been assigned as the cause of the phenomenon. The cones function in high degrees of illumination, are almost inexcitable by light of short wave-length, and secure the visibility of the red end of the spectrum, while the rods function in low degrees of illumination, are inexcitable by light of long wave-length, and secure the visibility of the violet end of the spectrum. Certain workers, however, have reported the phenomenon in the fovea itself, where we find only cones. Consequently another interpretation must be sought. Such an interpretation has been suggested on the basis of the Young-Helmholtz trichromatic theory. If there exist independent receptors for blue, with predominant excitability for radiations of high frequency, and for red, with predominant excitability for radiations of low frequency, it may be that the increment of the sensorial effect with increasing energy of radiation is not exactly the same for the two types of receptor, that the sensation of red rises more quickly than that of blue, and that the sensation curves cross one another.

It has been claimed that a phenomenon analogous to the Purkinje phenomenon is present in the case of smell, and the explanation is on the lines suggested, if the sensibility to the two odours is formulated in terms of the Weber-Fechner Law. It is not absurd therefore to think of the same principles as holding in the case of the visual receptors. This raises, however, fundamental questions regarding the validity of the Weber-Fechner Law. If one sensation increases more quickly than another in a certain range of stimulation, is this because the more acute differential sensibility involves in a certain range a greater number of steps, and is the step the general unit common to all the sensations? It is to answer these questions that the authors have carried out their investigations on the photometric equalization of monochromatic lights in the foveal area, and the value of the differential steps for these lights. The results of experiment appear to show a finer sensibility for red, a coarser for blue, and an intermediate for green. The sensation curves therefore are as suggested.

The most interesting series of experiments were those carried out with the stimulated areas falling within the fovea. The areas had to be small—subtending an angle of 48'. Absolute and differential thresholds were determined, and for both the threshold for red was approximately two or three times less than that for blue. The general conclusions at which the authors arrive are:—

(1) The Purkinje phenomenon is definite though reduced when the red and blue stimulations pass beyond the fovea, however little; fails entirely in the fovea itself; and is thus presumably to be explained by the duality of the rods and cones, without there being any need to appeal to a difference of sensation curves for different receptors.

(2) The problem remains to be solved of the individuality of the sensation curves for the receptors, which is implied by the trichromatic theory and by the Weber-Fechner Law.

(3) Near the absolute threshold the differential threshold behaves in a complex way. In place of rising constantly, as has been generally held, it seems to rise and then fall.

Of the two other technical papers by Piéron the more important psychologically is that on the analysis of reaction time. In this the author discusses some of the main theoretical implications of the results of the work done on reaction time by various investigators, including himself, together with work done by physiologists on nervous process and on reflexes. The interest of the psychologist, he points out, is mainly in the volitional phase of the reaction, whereas the efforts of analysis have usually been directed to other phases of the reaction. The earliest work in analysis was that of Wittich (1868). By stimulating different parts of the body with tactile stimuli he determined the rate of conduction in afferent nerves, and at the same time noted that the rate varied with intensity of stimulus. A little later Exner compared the reaction time for light, and for electrical stimulation of the eye, and showed that the latent time for light was from 30 to 40 thousandths of a second. He also noted the effect of intensity on reaction time. The influence of intensity was studied by Piéron himself in a systematic series of investigations between 1910 and 1914, in which light, temperature, tactile, and gustatory stimuli were employed. As a result of this work he was able to formulate a law holding for all the different kinds of stimuli, relating the time of reaction to the intensity of stimulus. This work, interrupted by the war, was resumed in 1919. This law must not be taken as the end of research, but rather as a starting point for the analysis of latent times. An examination of all the work done enables one, in Piéron's view, to separate out the 'irreducible latent time' in the receptors and in the synapses. He claims also that psychological work on reaction time and physiological analysis throw light on one another, and that the law which he has formulated represents the superposition of laws proper to the diverse processes which are involved in the reducible range, laws which have still to be determined. He concludes by pointing out that the classical reaction time methods have already enabled him to establish the existence of independent receptor systems for different kinds of pain, based on the different rates of conduction in afferent paths, and that this finding is confirmed by the work of Adrian and others.

The most interesting paper from the less technical point of view is probably that of Durup on hypnagogic phenomena. Durup refers first of all to previous work, and notably to work by Leroy and by Slight. The hypnagogic visions in his case are similar to those described by Slight, and he is unable to draw the distinction which Leroy draws between hypnagogic visions and dreams. Slight classifies the phenomena into three groups: (1) symbolic ideas originating in connection with ideas or states of the mind or body, (2) dream states occurring while falling asleep and of the same character as the dreams of actual sleep, (3) dream states in connection with objective stimuli. All Durup's observations belong to one or other of Slight's groups. Typically invention or sudden inspiration seems to be of the same order as these visions. In fact, hypnagogic digressions offer an opportunity for invention. The unexpected joining of two ideas, hitherto unconnected but nevertheless having some deep analogy or connection, may form the starting point for real creative work.

JAMES DREVER.

Opera hactenus inedita Rogeri Baconi, Fasc. XII : Questiones supra librum de Causis. Nunc primum editit ROBERT STEELE, collaborante FERDINAND M. DELORME, O.F.M. Accedit Liber de Causis, a ROBERTO STEELE denuo recognitus. Oxonii, MCMXXXV, e Typographeo Clarendoniano. Pp. xxiv, 194. 17s. 6d.

THE history of the so-called *Liber de Causis*, apparently first translated from the Arabic of an unknown compiler, who seems likely to remain unidentified, by Gerard of Cremona in the twelfth century, is a curious one. This *liber de expositione bonitatis purae* (to give it its earlier appellation) was at first supposed to be an authentic philosophical treatise of Aristotle, in spite of its undisguised Neo-Platonism. As our editors well point out the mistake had important consequences. The original title exempted the work from the censures passed in the first quarter of the thirteenth century on the newly recovered *Physics* and *Metaphysics*. Hence it was widely studied in the University of Paris, and must have given an unfortunate unhistorical bias to contemporary conceptions of Peripatetic philosophy. That the work as it stands is not Aristotelian was, indeed, early recognised, but the theory persisted—we find it apparently accepted both by Bacon in the present volume, and by Albert the Great—that the enunciations of the propositions are Aristotle's, though the *Comments* (or rather 'demonstrations') are a compilation from Arabic sources, such as Avicenna, Algazel, or Alfarabi. It was left for St. Thomas, who had the benefit of the Greek scholarship of William of Moerbeke, to state the truth, that the whole work is based upon a number of propositions excerpted from the *Elements of Theology* of Proclus, and adapted to the requirements of Moslem Theism.

(The present editors suggest that the work, in its Latin form, has perhaps undergone a double adaptation of this kind, first to Islam and then to Christianity. I feel a little doubtful about this. The transposition of the Neo-Platonic 'One' into a transcendent *personal* creative Deity is manifest enough, but I do not see any evidence in the text that there has been any further adaptation to specifically *Christian* theological doctrine. I may remark also, in passing, that it is perhaps a pity that Mr. Steele's language on p. xx inadvertently suggests that a comparison of the work with the text of Proclus has only become possible since the publication of the critical edition of the *Elements of Theology* by Prof. E. R. Dodds in 1933. There was no decently scholarly edition of the work of Proclus anterior to that of Prof. Dodds, but the text had been printed at least three times, by Portus in 1618, and twice by Creuzer in the first half of the nineteenth century. The work of Portus and Creuzer is very badly done, but it is quite possible, with the aid of either, to compare the substance of Proclus with the contents of the *De Causis*. Indeed, something of the kind was already possible to, and has been partly done by, St. Thomas, whose *Commentary* shows that he had the complete version of William of Moerbeke before him.)

It is not likely that any but special students will bestow much time on Bacon's lectures on this curious book, nor are they likely to be much profited by it. Intelligent exposition, indeed, was scarcely possible as long as a number of strictly Neo-Platonic theses were supposed to be the composition of Aristotle, and were consequently misinterpreted by attempts to deduce them from Peripatetic premisses. Nor do I think Bacon, in any case, had the kind of mind which would make him a valuable expositor of such a work. As in other reports of his lectures, here also he seems never to attempt any intelligent comprehension, as a whole, of the work he is ex-

plaining. He is content to take each thesis separately, start any difficulties which he can invent in connection with it from an Aristotelian point of view, and get rid of them, sometimes superficially enough, by some *frigida distinctio* borrowed from the Aristotelian logic. The sense for the general drift and purport of a connected whole seems to have been denied to him. In that respect he shows himself hopelessly inferior to St. Thomas, as might be sufficiently proved by comparing his *Questiones* with the Commentary of Thomas on the same text. I own to a doubt whether Bacon ever possessed, as St. Thomas eminently did, the "synoptical" vision which, as Plato has told us, is so indispensable to the philosopher, and I suspect that this is the real reason why his reputation as a thinker seems always to have been highest, in modern times, with those who have known least about his writings. I do not know whether many persons besides myself have had occasion to read the whole twelve *Fasciculi* of the *Opera inedita* so far published; those who have done so, I suspect, must, like myself, have found their estimate of the *Doctor Mirabilis* steadily growing lower and lower. And this cannot be completely explained by the reflection that no thinker is seen at his best in a mere collection of 'class-lectures;' for some of the lecture-courses of St. Thomas, as, for example, those on the *Ethics* and the *De Anima*, are of unmistakably first-rate quality. Bacon's mind—I regret to say it, but I think it ought to be said—compared with his is frivolous.

The text before us has been, like so much more, printed from the Amiens MS. Many of the worst errors of the scribe have been diligently corrected in the text itself: others find a remedy in the list of emendations prepared at the end of the volume. (I do not fully understand the principle upon which some obvious corrections are relegated to the Appendix while others, not more imperative, are admitted into the text. The editors clearly do not conceive it to be the duty of an *editio princeps* to reproduce all the errors of their MS. as it stands; yet, if any corrections at all are made, why should certain absolutely imperative corrections be given less favourable treatment than others? There seems to be some vacillation in the underlying conception of what an editor's duty in such a case is.)

In the main, so far as I can judge (speaking, of course, as one who is not a professional palaeographer, and has never seen the Amiens MS.) the corrections, both those introduced into the text and those placed in the Appendix, are judicious, and reflect high credit on the palaeographical side of the work. There are, indeed, a few exceptions. For example, at 13, 11 the *securis* of the MS., for which *universale* is arbitrarily substituted in the text, seems to me to be clearly right. The *securis* is Aristotle's ἀξίον, a standing example of an 'instrumental' cause. So the proposal in the Appendix at 94, 10 and 115, 18 to change *primitivum* to *privativum* is most unfortunate. In both cases Bacon means exactly what the MS. says, that the word *sui* may be 'either the primitive or the possessive' (i.e., that it may be the genitive either of *se* or of *suum*) and that the truth or falsehood of the proposition under discussion depends on the grammatical construction adopted. (In no sense whatever could *sui* be called 'privative.') 86, 14, the MS. *quia* seems apt as against the *quare* of the printed text; the clause gives the reason for what has just been said. 158, 23 the MS. *cum sit ex materia et forma sic compositum* is plainly right: the change of *sic* to *fit* in the printed text destroys the sense.

In the main, however, the corrections made by the editors seem to be thoroughly sound. The pity is that they have not made a good number of others, without which the text is more or less unmeaning. I propose,

therefore, to submit a number of such corrections, some of them, as I own, only more or less probable, but others made certain by context. 1, 28 facit se, *l.* facit (extra) se. 2, 1 facit se extra, *l.* facit extra se. 11, 34-5 *dele* recipiatur in causa secunda. 12, 1 minor, *l.* minus; 19, ix excessus, *dele* ix; 34 continuare, *l.* continuari. 18, 16 destruitur, *l.* (non) destruitur. 19, 26 singularitatis, *l.* singularis. 20, 7-8 set . . . manet, *l.* (s)et . . . man(er)et; 25 patet animal, *l.* patet quod (*animal* is an anticipation of the *animal* which follows in the same line). 21, 10 sic, *l.* sue. 24, 28 hoc est verum, *l.* hoc (non) est verum (as the sense requires). 26, 28, 32, 36 creare, *l.* creari (*ter*). 27, 1, 2, creare, *l.* creari (*bis*); 2 crearet, *l.* crearet(ur). 32, 9 (et formale), *dele*. 35, 19 fecere . . . non-esse, *dele* (a dittography from *l.* 17 *supra*). 49, 8 natura alia, *l.* aliqua. 50, 14 quare si, *l.* quare (ni) si. 51, 11 non intelligit, *dele* non. 53, 2 inferior; intelligentia, *l.* inferior intelligentia; 10 quare non? *l.* quare non etc.; 12 qui sit, *l.* qui fit; 19 sicut habet, *l.* sicut (se) habet. 58, 38 ut per hec, *l.* et per hec. 61, 10 offeretur, *l.* offer(r)etur. 63, 12 quod intelligentia, *l.* quod (in) intelligentia. 65, 30 secundam rem, *l.* secundum rem. 67, 33 non debet hic dicere, ? *l.* non debet hoc dici. 68, 35 excluditur, ? *l.* (non) excluditur. 69, 5 primo, *l.* imo. 70, 32 per presentiam, cum celum sit, *l.* per presentiam. (ITEM) cum celum etc. 74, 23 absolute, *l.* (non) absolute; 32 vel anime, *l.* vel (pars) anime. 75, 4 hoc modo per acquisitas, *l.* hoc modo, (sed nunc) per etc. 76, 16 exigentia, ? *l.* existentia. 81, 20 omnis regressio fit ad habitum, *l.* ad non-habitum (as the reasoning requires). 82, 14 ab eodem idem, *l.* ab eodem (ad) idem. 88, 23 quam non quam magis infinita, *l.* quam non quam magis (unita) infinita. 92, 2, creatura, *l.* creata. (The meaning is 'it would droop away immediately after its creation.') 93, 7 scilicet, *l.* set. 95, 38 (first word) infinita, *l.* finita. 96, 12 ad creatum, *l.* ad creatorem. 99, 7 ita consideratur, *l.* (v)ita consideratur. 102, 2 ipse ad minus, *l.* specie ad minus; 12 de solo, *l.* de suo. 107, 2 concreare, *l.* conservare. 109, 23 celorum, celorum a, *l.* celorum, a. 111, 15 quando, *l.* quare. 117, 35 intra, ? *l.* (ab) intra. 119, 3. The words *alicuius corporis* are intolerable here; perhaps their right place is in *l.* 2, motor et actus (alicuius corporis). 125, 29 natura omnium, *l.* materia (*cf.* *l.* 36, below). 126, 1, 4, et, *l.* ut (*bis*); 37 illa substantia, *l.* nulla substantia. 129, 16 nec, *l.* et. 137, 22-3 potest primi via, *l.* via (argumenti). 138, 15 prima est, *l.* prima (quod) est. 140, 32 aut non, *l.* aut(em) non, and remove the punctuation mark before autem. 142, 25-6 communicant tantum enim est. This is unintelligible. *L.*, probably, communicant tantum. (et) enim est. 143, 33 *dele* one of the occurrences of the word *numero*. 145, 14, nobilis, *l.* (intermi) nabilia, 147, 28 in the words supplied by the editors *dele* the non. 148, 2 unigenitas, *l.* unigeneitas (*cf.* *unigena*, *l.* 34); 15 res, *l.* rei. 149, 20 *dele* a mensuratione; 35 qua, *l.* que. 153, 14 mensuretur, *l.* mensurantur; per essentiam, *l.* (su) per essentiam. 156, 22 videtur quod sic; *l.* (as the context demands) quod non. 157, 19 creatum hujusmodi, *l.* creatum (in quantum) hujusmodi. 158, 17 simplex similiter, *l.* simplex: similiter. A few of these suggestions may be doubtful, but most of them, I feel sure, will be seen to be necessary by anyone who reads the text with attention to its meaning. I hardly think the University of Oxford should expect the reader to do all this correction for himself in what it issues as the work of one of its most famous names.

A. E. TAYLOR.

P.S.—I have had to read the proof of this notice without access to the text. I trust no errors have crept in, but if there are any, I apologize for them.—A. E. T.

Essais d'Esthétique de Philosophie et de Littérature. By VICTOR BASCH.
Paris: Félix Alcan, 1934. Pp. viii + 411. 50 fr.

THIS selection of M. Basch's contributions to different periodicals was presented to him by some of his pupils on his retirement from his distinguished career of 48 years of teaching, including 27 at the Sorbonne. The second and third parts contain articles on Kant's centenary, on the place of the imagination in the Kantian theory of knowledge, on Hegel's political philosophy, on Ernest Renan, on Søren Kierkegaard, on M. Brunetière's ideas, and on the relations between Ibsen and George Sand. Here only the first, the æsthetic, part will be reviewed, partly for the sake of unity, partly because, valuable and extremely interesting though they undoubtedly are, the rest are largely expository.

The first article, published in 1920, a year after the inauguration at the Sorbonne of the Chair of Æsthetic and the Science of Art with which the author was himself entrusted, is a programme of studies for Æsthetic. The latter is conceived as an intermediary science—Hegel's *Mitte*—between Psychology and History, using, like Psychology, observation and experiment, and reconstructing like History, but also sharing the normative character of Logic and Ethics; it is to be carried on by three "guilds" of researchers—philosophers, historians of art and artists. Its field is shown to have been vastly extended in our own days, in the direction chiefly of anthropology, ethnography, ethnology and sociology, by the genetic method. Those who are inclined to look upon the latter as *πολυμαθία* which is not *πολυνοΐα*, as a scampering through mainly fabulous stages of history which replaces rather than controls thought, and upon its contributions as Danaan gifts, will not be reassured by M. Basch's exposition, in spite of its admirable lucidity and firm grasp of connections. At one time he states (*cf.* p. 22) that light is to be sought from the whole process, including the most developed stage—an incontestable assertion. At other times, however, he maintains (*cf.* p. 20) that the essence of artistic creation is best revealed in its most primitive manifestations. Such a contention is consistent with the assertion of the second article (p. 37) that the æsthetic sentiment, the æsthetic phenomenon, is to be found (as much, presumably, as in the enjoyment of real works of art) in the appreciation of a stupid novelette, pornographic music-hall turn or cinema thriller; but it is extremely controvertible. However that may be, M. Basch himself does not show us how these new enrichments help with what he considers the last and essential problem of Æsthetic, its Metaphysic, the question of the place of Beauty in Nature and of Art amongst the manifestations of the human mind. Indeed modesty must preclude him from trying, for he states (p. 22) that "a monstrously encyclopædic brain" will be required to synthetise the results yielded by the genetic method. But one thing we may assert certainly, if somewhat frivolously, that where a State undertakes to carry through adequately the study proposed by him, requiring whole "peoples of researchers", there need be no fear of unemployment, intellectual or even manual, especially as a considerable place is assigned to the classification of the arts, susceptible, surely, of an infinite number of modes, though he himself suggests only a few.

In the second article, *The Chief Problem of Æsthetic*, published in 1921, he gives us the fruit of all his reading and reflection since 1895: it is the palinode to his *Essai critique sur l'Esthétique de Kant* which appeared in 1896 and which assigned too pre-eminent a rôle to feeling in the æsthetic

activity. Here an attempt is made to do greater justice to the intellectual or cognitive element in it. It is now acknowledged to be contemplation and therefore a kind of knowledge, but only of external sensible appearances or images, their associations and their forms, not penetrating, like knowledge proper (science), to the essence, nor, like it, grasping the general, but sticking to the individual, and that too an individual absolutely isolated in a light, unreal dream or play world; it is a creative intuition, a purely subjective contemplation emanating from our good will, caprice, momentary disposition, and the æsthetic sphere is created by us alone. Where this is not misleading, it is truistic in a way in which it need not have been if an attempt had been made to reconcile it with the opposite assertion, often made and obviously containing some truth, that not science, but only the æsthetic imagination, penetrates beneath the surface to the essence not only of the individual but also of the universal. Indeed the problem of æsthetic cognition, so it seems to me, is wholly distorted by a presentation like that of M. Basch. The æsthetic experience is cognitive (it is a contemplation or acquaintance with), not, however, of this tree, or of the generic tree, nor even of this æsthetic artifact as a physical object, but of this quality, atmosphere or spirit dwelling in a *locus* constituted, *inter alia*, by images of, and defined by causal reference to, this tree, or the generic tree, or this physical artifact. It is subjective only because the subject, or that which is closely dependent upon him, is a part-constituent of the *locus*, though not of the substance, of the ultimate æsthetic object, *i.e.*, of the *locus* of the quality, atmosphere or spirit; otherwise it is as objective, compulsive, authoritative as anything can be. If in *his* sense it is true (in one sense, I admit, it is true) that anything can become an æsthetic object provided we contemplate it (p. 51), what does M. Basch mean when, in the first article (pp. 30-31), he suggests after Kant that a completely beautyless Nature is conceivable? It is obvious there that he does not mean simply an un contemplated Nature. And if the æsthetic activity is so much a matter of our caprice, what of the self-abnegating discipline to which the artist and the contemplator must alike submit? It is, so it seems to me, the monadism, rather than any supposed superficiality or capriciousness, of the æsthetic experience that could yield really fruitful questions. But M. Basch finds the main æsthetic problem in the feeling which accompanies æsthetic contemplation, and the answer to it in the *Einfühlung* theory, which he briefly presents with so smooth a surface that one would scarcely suspect that there is a lake beneath which lurks hydra-headed Difficulty.

In the next article, *The Expressive Power of Music* (1926), the problem of feeling is taken up once more. Music, taken by so many for the very language of feeling, cannot, we are shown after Hanslick, express *directly* any affective content: play Bach's *Die Freude ist in Gott*, without giving the title, to people unacquainted with it, and to some it is a sad dance, for others it evokes Versailles of the Pompadour period, and for others again mortal torments. (Though one agrees with M. Basch's thesis one cannot help thinking that the interpretations he cites are not so disparate or conflicting as he would have us believe.) Music expresses only the dynamism of feeling, modes of movement and of direction of forces—agitation, calm, hesitation, palpitation, tension, relaxation—with which become associated more definite affective states: disquiet, joyful serenity, vital willing, melancholy. These latter are still only directions of feelings and not the feelings themselves, the *terminus* of which is supplied differently by different listeners: I hear rising sound and my soul rises, but the

"rising" is to the face of the beloved, or in society, or of my shares, according as I am a lover, social climber or financier (pp. 74-75; cf. p. 77: a "calm" phrase may evoke calm after storm, a woodland glade, the façade of a Greek temple, a gentle and serene face). More exactly, music expresses directly only itself—the dynamism of the movement of sounds which is closely analogous to the dynamism of the movement of our states of consciousness. Given the psychological mechanism by which every strong feeling tends to express itself in sound, and we can see how the analogy comes to be interpreted as identity and music becomes the favourite domain of *Einfühlung*. Apart from the statement about *Einfühlung* and about the supposed transformation of analogy into identity, M. Basch's observations might be extended to all art, including poetry which professes verbally to be "about" something very particular, a particular lovely woman, or love or fighting; they might also explain, partly at least, the universality of art. With regard to music they receive striking confirmation from attempts to interpret it by means of the cinema: only the generality of geometry is here adequate to the generality of music—dancing dots, lines, curves, geometric patterns—while natural shapes, however fleeting and in however chimerical combinations, seem simply irrelevant.

Yet, to judge from the article entitled *Romanticism in Music* (published in 1929), the affective content of music is susceptible of fairly precise description: M. Basch is able to define romanticism in Beethoven, Weber, Schubert, Mendelssohn, Chopin, Schumann, Berlioz, Liszt, and Wagner as aspiration after the infinite and striving for aggrandisement, as preference for feeling, for the subjective and the particular, for exaltation, melancholy, nostalgia, tempest, battle without victory, as the individual's sense of complete liberation bringing first inebriation and then disillusionment, *Weltschmerz* and the longing for escape into the distant, strange, mysterious. The description is done well and convincingly; indeed, if anything can allay the suspicion that "classical" and "romantic" are mere pseudo-concepts, it can.

Finally, in *The Origins and Foundations of Hegel's Aesthetic* (1931), M. Basch represents Hegel as summing up and carrying to its furthest point the æsthetic tradition which, beginning with Plato and Plotinus, came to him through Kant, Schiller and Schelling, the last being the follower but also the transformer of Fichte. He concludes with the affirmation of his conviction, reached after prolonged study of the chief hypotheses on the nature of Beauty, that, even as this tradition asserts, Beauty is something like "an appearance of the Idea", a sensible incarnation of the intellectual, and that the function of the æsthetic is that marked out by Hegel, to reconcile the sensible world with the intelligible, necessity with freedom, Nature with Spirit.

P. LEON.

Grundlagen der Mathematik, vol. i. By D. HILBERT and P. BERNAYS. Berlin: J. Springer, 1934. Pp. xii + 471. M. 37.80.

THIS book is intended to be a first instalment of a full account of investigations into the foundations of mathematics initiated by Hilbert in 1904 and since pursued by the so-called 'formalist' school of mathematical logicians. In England, the views on which these researches are based have been regarded hitherto with a kind of respectful astonishment. Any theory which leads to consistent and successful research during so

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many years is not to be dismissed without careful examination, but the very few English critics who have reported on the formalist position have done so little to make it appear more than an aberration from the doctrine of *Principia Mathematica* that justice and plausibility must surely have been sacrificed.

For these reasons, an authoritative account of formalist views is welcome to all who are concerned with the relation of mathematics to philosophy and the sciences, and wish to hear a doctrine alternative to that of Whitehead and Russell. Unfortunately, general discussion of the formalist theory occupies less than a tenth of this volume (44 pages); the rest is devoted to the minutiae of technical definitions and proofs, so that the whole has the appearance of an ill-arranged text-book for the converted specialist. The latter will be glad to find classical proofs more conveniently accessible (even at an exorbitant price), but most readers of *MIND* are likely to be more interested in the introduction, which ought to be noticed in more detail.

The Introduction : On the basis of a description, in some detail, of the 'axiomatic method' as used in mathematics and the sciences, the following assertions are made: (1) the theorems and axioms of a theory in strictly axiomatic form contain exclusively variable names for objects and relations (i.e. propositional functions but no propositions), (2) a pure or 'formal' theory of this kind may admit of many alternative interpretations, (3) the internal consistency (*Widerspruchsfreiheit*) of such theories may be examined without reference to any interpretation of the theory, and, indeed, (4) most scientific theories are an 'idealised and simplified' account of their subject matter, are 'extrapolated beyond the possibility of verification', and cannot be supported, beyond a certain point, by appeals to observation (p. 3). Hence, (5) evidence used in examining the consistency of theories must ultimately be of a kind more 'primitive' than that expressed by any interpretation of the theory itself. It is pointed out that the formal structure of scientific theories is exhibited by a *mathematical* interpretation, and the method employed is illustrated at length by the arithmetical interpretation of a geometrical theory (pp. 4-13).

Having now reached the ground floor, as it were, the authors examine the nature of mathematical or rather arithmetical, theories. They assert: (6), Theories are easily produced which cannot be interpreted by any finite set of numbers but could be satisfied by an *infinite* 'set'. (7) Infinite 'sets', 'collections', or 'totalities' cannot be discovered by experience (hence, e.g., the rejection of Russell's axiom of infinity) and involve an extrapolation of the sort above mentioned (4). Hence, (8), the system of mathematical analysis, which irreducibly involves notions of infinite sets, sets of infinite sub-sets, etc., needs a proof of consistency (Frege's authority is quoted in support), and (9), the method of *Principia* is unsatisfactory since it uncritically uses infinite collections, if not of numbers, then of predicates or propositional functions. [In this chain, (7) is the weakest link: the word 'extrapolation' remains an unreduced metaphor. The whole treatment needs elaborating.]

Thus, as Hilbert has made clear in other papers, the notion of infinity is regarded as an 'ideal' in the mathematical sense, i.e., a symbol whose introduction helps to exhibit similarities of structure between related systems, and simplifies proofs, but whose 'meaning' is exhausted by this kind of operative function.

The programme which emerges from these considerations is nothing less than the detailed examination, for internal consistency, of the entire

system of pure mathematics. For this purpose, the theories examined must be put into strict axiomatic form, *i.e.*, must be symbolised without remainder by the combined use of mathematical and logicistic notation. In view of what has previously been said, ((1) and (2) above), all reference to the 'meaning' of the symbols can then be eliminated and pure mathematics can be treated as a set of physical objects (marks on paper) combined and permuted according to specified rules. [Hence the popular but oversimplified formula that formalists regard mathematics as a game devoid of meaning.] The study of the formal properties of mathematics conceived in this fashion is called metamathematics whose informal, uncoded, procedure is carefully distinguished from its completely formalised subject matter. Metamathematics may itself need to use *some* mathematics (if only to count) but any results used must be 'primitive' (*cf.* (5) above).

The account supplied of 'primitive' or 'finite' arithmetic is not altogether clear. We are told that it is characterised by the use of experiments in the imagination (*Gedankenexperimente*) on 'objects assumed to be concretely presented' (*Gegenstände, . . . die als konkret vorliegend angenommen werden*). [One would like more information on the implications of the crucial word *assumed*. Would a person with a defective visual imagination be incapable of doing simple arithmetic? And is it not dangerously near to the mistaken view of empiricists like J. S. Mill to make even the simplest arithmetic the product of observation?]

For primitive arithmetic a single initial object is needed, say the cipher /, a generating process for appending (*anhängen*) further ciphers to obtain the series of figures

/ // /// //// etc.,

and further signs (the Arabic numerals, signs for multiplication, addition, etc.) for referring to the basic ciphers or operations upon them. The metamathematician may use mathematical induction for the recursive definition of simple functions. This kind of mathematics may seem questionably primitive; as a phenomenal description of simple arithmetical reasoning the treatment is altogether too idealised, and without such a basis the boundary between primitive and extrapolated mathematics, however definite, must remain somewhat arbitrary.

Yet it can hardly be objected that the limits of primitive arithmetic are chosen with less than an austere rejection of convenience. For it is shown in detail that, while sufficient materials are provided for the practice of a certain amount of algebra, to define functions as complicated as the exponential for positive integral indices or to prove the existence of infinitely many primes, existential statements about integers must be handled very delicately. They are regarded as fragmentary statements, *viz.*, as incomplete prescriptions for the exhibition of integers with specified properties. With such an interpretation, of which I have been able to give only the barest outline, the finitist is unable to practise mathematical analysis; some pains are in fact taken to show that the latter involves the fundamental abrogation of the 'finite' standpoint (pp. 36-42). In short, 'finite' or primitive arithmetic is based on accepting the demands of Brouwer and the intuitionist school, but in an even more radical form (p. 43).

It is a pity that the fundamental issues, ((7) and (8) above), are dismissed so summarily; the frequent use of incurably vague words like evidence, extrapolation, or intuition (*Anschaung*) is very disturbing and contrasts unfavourably with the precision of the later part of the book. It is much

to be hoped that Prof. Bernays or some other member of the school might be persuaded to issue the introduction separately in a much expanded form.

A very brief mention must suffice for the detailed proofs in the remainder of the book. The treatment of the propositional calculus is a great improvement on previous accounts and contains much interesting work which has never been brought together before. Elementary cases of the solution of the *Entscheidungsproblem* are discussed (the proofs being often simplified or improved), and the functional calculus is developed very carefully. Much of the text is concerned with systematising and adequately symbolising the mathematical theory of functions in preparation for the difficult proofs of *Widerspruchsfreiheit* which are to be expected in the second volume. All this is treated with great stringency.

There is a serious defect in presentation. No book of this kind ought to be written in continuous narrative. When chapters are over 20 (4to) pages in length, a table of contents is no substitute for a complete absence of cross-heads in the text.

M. BLACK.

A New Argument for God and Survival. By MALCOLM GRANT. London: Faber & Faber, Ltd., 1934. Pp. 450. 12s. 6d.

THIS is an interesting book partly because of its subject and partly because its author is so independent a thinker that he avoids both scientific and religious prejudices. I may assure those who are sick of the feeble apologetics of the present age that here is something neither vague nor spiritualized. Mr. Grant says plainly what he believes and why.

Roughly speaking Mr. Grant seeks to prove God's existence from miracles, and immortality from God's existence and the special nature of the miracles.

By a 'miracle' Mr. Grant means an event which is not caused naturally. An event is caused naturally when it is brought about by a natural cause in accordance with a law. He does not make very clear what he means by 'natural cause'. An event in the history of the material things, animals and persons around us would be a natural cause. But what about an event in the history of a discarnate spirit which now and then possessed some organism?

The first main position held in this book is that we might know (a) that if a certain event took place then it was not caused by a natural cause in accordance with a law, and know (b) that the event took place.

There is a conflict between those who deny miracles. Some deny the occurrence of the events and some their miraculousness. Some say "Christ did not rise, for that would have been a miracle"; others say "Christ rose, so his resurrection was not a miracle".

If one were to reply to Mr. Grant that in such cases the reasonable thing is to hold an alternation *Either he did not rise or his resurrection was naturally caused*, then he would say: The evidence against both alternatives might be so strong that it would be better to accept a miracle.

I think we ought to agree that this *might* happen. We must allow that the evidence against there being some hidden law might grow stronger; for we could not establish laws of the form *S is the cause of P* unless we could sometimes render more and more improbable the presence of some hidden cause of P. And we must allow that the evidence for the occurrence

of an event might grow stronger and stronger. Therefore unless we assume it *certain* that all events are caused by natural causes in accordance with a law, there will come a point when it becomes best to allow that an event was not so caused, that is, was a miracle.

Mr. Grant believes that from miracles the existence of a single, intelligent, non-natural cause can be inferred. Like all events the miracles must have causes. Could they be caused by natural causes, though still miraculous because not caused in accordance with a law? Yes (p. 63, l. 4). But we could never *know* that an event was so caused. For we should never be confident that a man was responsible for a miraculous cure, say, unless he produced such cures so regularly that we should infer a law.

There seems to be some confusion here. It is true that we should not have sufficient evidence to show *which* natural cause caused an event without inferring a law connecting it with the event and thus precluding the event from being a miracle. But this need not prevent us from claiming that the best hypothesis about the causation of a certain event is that it was caused by *some* natural cause though not in accordance with a law.

It would have been better to argue that causation not in accordance with a law is absurd. From this would follow the desired conclusion that a miracle requires a non-natural cause or causes.

Mr. Grant's attempt to justify his belief that in general it is better to suppose a single non-natural cause than many, seems to me fallacious. However, as he says, the special nature of a set of miracles might render a single cause more probable.

The next step is important. Mr. Grant maintains that we not only might know of miracles but do know of some. The extraordinary phenomena reported by the Society for Psychical Research and by Spiritualists, the cures reported from Lourdes and by Christian Scientists, are miracles. With very interesting instances Mr. Grant shows that the evidence for the first mentioned group of events is very good. He points out that for a long time first-rate minds have failed to find any laws for these events. He claims (1) that it has now become clear that the events must be admitted and that they are best explained by the hypothesis that they are caused by the intelligent volition of a single non-natural being of immense power; and (2) that careful consideration of the miracles and of the world suggests that this being is benevolent and therefore properly called 'God'.

This hypothesis Mr. Grant seeks to support by an explanation of how we might attribute to God purposes which would make his intervention not merely the only remaining possibility but a rationalization of the occult, in the sense of a hypothesis which would enable us to explain the many puzzling details it presents (see pp. 117 and 427).

The miracles, it is alleged, have served several purposes, amongst them, the proof of God's existence to careful thought, the production of new religions and the preservation of a belief in the unseen and survival. The miracles have been random and at times "hellish" because God wished to prevent his authorship of them from being obvious, and to allow scientists to disbelieve in them and thus develop the excellent results of their faith in the uniformity of nature. "The difficulties in miracles are calculated . . . so that a solution can only be arrived at after a certain course is run." I cannot here do justice to Mr. Grant's explanation of God's purposes in making miracles mysterious. It involves his theory of revelation, according to which God may well "reveal" things which are not the case, for purposes of his own.

The theory is applied in detail to ghosts, faith-healing and thought-reading, but the exposition becomes rather confusing.

The argument of the book seems to me to force us to weigh the probability of an intervention by a God against the probability of a hidden law. I retain my faith in the hidden law. Many events were at one time thought to be due to non-natural causes—eclipses, this and that disease and cure. The experts still do not know the causes of cancer, epilepsy, schizophrenia; yet no one believes these disorders to be miraculous. It is true that the phenomena, for example, materializations, which Mr. Grant claims are miraculous are more likely to be so than any other well-established events. But even here, can we confidently argue that there is no law behind the phenomena because the experts have so long failed to find one?

As Mr. Grant points out, his argument from miracles might well be combined with an argument from design.

In the matter of God's existence it is difficult to be sure that we are neither overestimating evidence because of a desire for his existence, nor underestimating it because of a fear that someone will think we are "going over", to use a phrase of Mr. Sprott's. Put it this way: If our lives depended on being right, should we say that God exists or should we say that God does not exist? I should choose the negative, but nervously, and the more so for Mr. Grant's book.

J. WISDOM.

A Common Faith. By JOHN DEWEY. Yale University Press, Newhaven, Oxford University Press, Humphrey Milford, London, 1934. Pp. 87. 7s. net.

WHEN a philosopher of Dewey's standing is moved to define his attitude towards the religious problem, he is assured in advance of a respectful hearing, and Yale University has deserved well of the republic of letters by tempting him to deliver these Terry Lectures, of which the topic is stated to be 'religion in the light of science and philosophy'. The result is an impressive and 'meaty', though by no means facile, argument, set forth in simple and direct (though rather abstract and austere) language, without any meretricious adornment.

Prof. Dewey here undertakes to mediate between the adherents of positive religion, for whom belief in a supernatural being is essential to religion, and those who feel they must reject the supernatural in all forms, and therefore think that all religion has lost its meaning for them. Dewey sets himself to cater specially for the latter, and to persuade them that they can be religious without having a religion, because the essence of religion is a social attitude which recognises that "we who now live are parts of a humanity that extends into the remote past, a humanity that has interacted with nature". On this solidarity of man with man and with nature can be based "a religious faith that shall not be confined to sect, class or race". Such has always been "implicitly the common faith of mankind. It remains to make it explicit and militant" (p. 87).

To reach this 'positivist' conclusion Dewey starts with an anthropological survey of religious phenomena, which reveals that all religions have been relative to social conditions and to social fears of a world that could not be controlled, and asks whether that is not now as true as ever. He points out (p. 20) that moral faith is practical in intention and that "the inherent vice of all intellectual schemes of idealism is that they

convert the idealism of action into a system of beliefs about antecedent reality" (pp. 23-24). So (p. 28) "the opposition between religious values as I conceive them and religions is not to be bridged". Accordingly, he proceeds to discount the psychological argument from religious experience and mysticism. It fails to distinguish between the fact to be inquired into and the interpretation put upon it (p. 35 f.), and to see that science is essentially method not doctrine (p. 39). Accordingly a question arises about the meaning of 'God'. Is God "only a particular Being" (p. 42), or "the unity of all ideal ends", and "a unification of ideal values"? (p. 43). Dewey holds that the latter is enough, and more than enough. It avoids the problem of evil which the definition of God as omnipotent renders insoluble (p. 45). In any case an existential God is postulated merely "in the interest of the supernatural" (p. 46), and this is wholly noxious. For "it diverts attention and energy from ideal values and from the exploration of actual conditions by means of which they may be promoted. . . . Men have never fully used the powers they possess to advance the good in life, because they have waited upon some power external to themselves and to nature to do the work they are responsible for doing" (p. 46). So Dewey admonishes us to trust in our own powers and those of nature, and to work together to realise our 'ideal values', thus taking 'God' as our active endeavour to realise the ideal.

Manifestly, Prof. Dewey's theory is striking and suggestive. It suggests also a number of questions. Before we can co-operate for the sake of values must there not be agreement about values? And how likely is it that we shall arrive at it?

Again, is the notion of the 'supernatural' distinct enough to serve as the true antithesis to a 'natural' religion? Is not 'nature' one of the most elastic of words in the philosophic vocabulary, equally capable of excluding and of engulfing, both men and supernature, if required? When Dewey says (p. 69) "in earlier times what we now call the supernatural hardly meant anything more definite than the extraordinary" and "in the older cultures the idea of the supernatural was 'natural'", is he not admitting this difficulty? And is it wholly true that belief in supernatural support merely paralyses? Does it not often prompt to more confident and bolder action?

As for 'God', what limits are there to the number and variety of gods, philosophers and theologians have dared to create by definition? Dewey puts all his money on "the mysterious totality of being the imagination calls the universe" (p. 85). But he makes no attempt to show that being does actually form a 'totality'. How then does he assure the existence of his deity by his speculative act of faith? Has he not still to prove that his 'imagination' will work, and that reality will obligingly conform to it? In other words, is the real really such that the conception of 'universe' will apply to it? Is not this essentially the issue raised by the notorious 'ontological proof'? I cannot see that Prof. Dewey is more successful in establishing it than the most antiquated apologist of theism. For it seems vain to plead that "the reality of ideal ends and values in their authority over us is an undoubted fact" (p. 44). A fact, no doubt; but how more than a fact of human psychology? The next sentence is stranger still; it reads like a desperate relapse into pre-pragmatic dogmatism. "The validity of justice, affection and that intellectual correspondence of ideas with realities that we call truth, is so assured in its hold upon humanity that it is unnecessary for the religious

attitude to encumber itself with the apparatus of dogma and doctrine". Is 'validity', then, a guarantee of working, and is 'intellectual correspondence of our ideas' a proof that reality will not put them to shame? Or can it be that our good Homer has nodded?

Lastly, let me say a word on behalf of the individual soul, presumably superior to the average, that is surfeited and sick of social rites and ceremonies. He has been bred and fed up on them, and has found them neither palatable nor sustaining. They seem to him obsolete and barbarous, and indeed at bottom devil-worship. What use is it to preach conformity to him? What comfort can he find in religion *à la* Dewey?

F. C. S. SCHILLER.

Nennfunktion und Wortbedeutung: Eine Studie über Martys Sprachphilosophie. By LUDWIG LANDGREBE. Halle: Akademischer Verlag, 1934. Pp. 132.

THIS book should be welcomed in so far as it can be taken as a sign of increasing interest in Marty's writings. And there is no doubt that it deals with a real difficulty in Marty's theory of names. What is more doubtful is whether Dr. Landgrebe's own suggestions do much to solve this difficulty.

The central theme of the book arises from Marty's account of the so-called "discrepancy between meaning and naming" which appears when a subject noun and predicate noun in a statement may be said to name the same thing but not to mean the same. Dr. Landgrebe thinks that Marty's account of the "naming function" in such connections is faulty, and that this fault leads to certain "gaps" not only in his theory of names but also in his theory of syntax. Dr. Landgrebe calls attention to the obvious distinction (to which I do not think Marty's theory was oblivious) between "naming" in the sense of "giving a thing a name" and "naming" as it is spoken of in referring to the "discrepancy between naming and meaning". When "naming" is taken in this latter sense Dr. Landgrebe identifies the "naming function" or as he sometimes calls it the "naming relation" (Nennbeziehung) with the predicative function or predicative relation. And there he leaves it. I should have liked some more detailed account of the "predicative function" together with a discussion of Marty's theory of predication. But apart from saying that the "predicative function" is a function which a name can have only in a judgment, and that it is a relation of the name "auf ein bestimmtes Ding" (p. 71), Dr. Landgrebe tells us nothing further of what he takes the "naming function" in this sense to be. All that is clear is that he is *not* using "predicative relation" in the sense in which Marty used it; for Marty spoke of a predicative relation only as holding between the different acts of a predicative judgment and never as a relation between a name and "a particular thing". Consequently Dr. Landgrebe does not make clear in the least what account he proposes to give of the "discrepancy between naming and meaning" in predicative statements.

Dr. Landgrebe seems to think that this sense of "naming function" (= "predicative function") is connected in some way that is not clear to me with "naming" in the sense of "giving a thing a name" ("das ursprüngliche Benennen"). And it is the naming function which arises in connection with giving a thing a name which he takes to be the more

fundamental. He says that it is through naming in this sense that names acquire meaning, and that the meaning they acquire in this way determines what they can name in the predicative relation. But a word cannot acquire meaning until it is used consciously as a name. This means for Dr. Landgrebe that it cannot acquire meaning until there has been a conscious decision (Entschluss) to connect it as a name or sign (Zeichen) with a particular thing. For we cannot understand a name or know what it means unless we are at the same time conscious of it as a sign designed to mean some thing. In other words, we cannot understand a name unless we understand its naming function, or at any rate are conscious of it as having a naming function. And this naming function is a relation between the name and the thing named by it which is established by the decision referred to. Dr. Landgrebe apparently takes this to be some sort of direct relation between the name and the thing, and not any relation of association in the minds of those who hear and use the name. But I cannot gather from anything he says what sort of relation he takes it to be.

This view of what is involved in the understanding of a name is one which Marty would not have admitted, and I cannot see that Dr. Landgrebe succeeds in showing that Marty is wrong. Nor do I think he succeeds in showing that the recognition of the establishment of a naming function in this sense is necessary for any account of the function and of the origin of syntax. In fact I cannot see how it could have anything in particular to do with the development of syntax. Dr. Landgrebe's attempt to show that it does, leads him to extravagances such as the statement that when a child first uses a word as a name of something the child must recognise the thing named as something constant and independent of its momentary wish for or dislike of it; and that "since the child recognises the thing as something in itself (für sich) and thereby *distinguishes* its presentation of the thing from the other acts of consciousness which may be connected with it, it is just through this distinguishing that the child becomes clearly (deutlich) conscious of that which, in distinction from the presentation and its expression, the name, finds its expression in the assertive or emotive statement".¹ I cannot believe that the child does anything of the sort. But Dr. Landgrebe thinks that it is this sort of reflection which enables the child to use parts of speech in their syntactical functions.

Dr. Landgrebe's criticisms of Marty's account of syntax are based almost entirely on misunderstandings of what Marty said, and in particular of the reasons for which Marty emphasised the rôle which the disinclination to use new signs when an old one can be put to new uses has played in the development of syntax. Marty emphasised this in order to refute the doctrines of writers like Steinthal, who held that the syntactical articulation of sentences must be strictly parallel to the structure of the thoughts they were intended to express. In this connection Marty's arguments are relevant to an account of the actual function of syntax as at present employed.

It is often difficult to see what the point is that Dr. Landgrebe is trying to make, largely because frequently when he seems to be professing to offer a criticism of Marty he is in fact simply repeating what Marty has said, with no indication that he is doing so. This holds especially of the section in Chapter III., where he claims to be giving a criticism of Marty's theory of "synsemantica"

¹ P. 111.

Morals and Politics. By E. F. CARRITT, Fellow of University College, Oxford: University Lecturer in Philosophy. Oxford: At the Clarendon Press, 1935. Pp. 216. 6s. net.

HE would be very undiscerning indeed who failed to recognise the merits of this little volume. It is crammed full of good things, and contains more material than many a volume treble its size. Mr. Carritt has something worth saying, and knows how to say it, and, for a lucid and sane treatment of the thorny question of political obligation, his book is unquestionably among the very best in our language.

The position that he is concerned to maintain is that 'no tenable political philosophy can deny the reality of obligations and responsibilities, and secondarily that an obligation to obey the laws of our state is not a special obligation, but only derivative from and dependent on obligations to our fellow men' (p. 198). And so, in a very happy introduction, he gives a concise summary of the main theories of morals and of the political theories based on them. He then devotes the first part of the book to a critical exposition of the views of Plato and Aristotle, Hobbes, Spinoza, Hume, Rousseau, Locke, Kant, Hegel, Green and Bosanquet, and seeks to show that, with the exception of Locke and Kant, all these writers have certainly failed to distinguish duty from interest, and consequently have no room in their systems for obligation in the moral sense. It is true that most of the above writers combine two or more distinct theories in their accounts of political obligation, yet at bottom they all continue to assume that the sole motive to obedience to the State is interest. In the second and shorter part of the book we are offered a discussion of the philosophy of history as illustrated in Hegel and the Dialectical Materialists (Marx, Engels and Lenin), since these writers have thought that such a philosophy has a bearing on the question of political obligation. This is followed by an analysis of political rights and duties, and a concluding chapter dealing with the General Will and the Contract.

We may say at once that the historical part is a model of sound and clear exposition, and enables the reader to see at a glance the vital points of the various arguments. I also heartily concur in Mr. Carritt's strictures on the philosophy of history. No more than he can I see the relevance of such a study to the question at issue. When even empirical history has so precarious a basis it is hard to see what useful purpose can be served by the speculations of a philosophy of history. Perhaps we had better pass over in silence the doctrines engendered by such a philosophy, *e.g.*, the General Will and the glorification of the State, though they still continue to trap the unwary.

When we turn to Mr. Carritt's direct analysis of our moral consciousness our unstinted admiration is tempered with a slight feeling of dissatisfaction. We are presented with a welter of moral judgments, without any attempt at unification. The Utilitarian principle, we are told, covers some of our duties, but not all; *e.g.*, the admitted duty to be just is not always reconcilable with the principle of most good. In a case of conflict of duties we ought to choose the strongest, or each in proportion to its strength. I do not pretend to know what this means, for no hint is given how we are to determine the true strength of our duties. We are certainly in need of some further guidance at this point. Moreover, I could wish he had expanded his reiterated assertion that the mere awareness of duty, apart from all desire, can move us to act. Presumably our sense of duty supplies

the emotion that enables us to do the right as right, but we are not told whether this sense is innate or acquired. Mr. Carritt will no doubt excuse his all too brief treatment of these important questions on the ground that it is not his intention to offer a complete moral philosophy at this stage, but I am afraid that his readers will feel that he cuts off his argument at an unfortunate point. I regret this all the more as I feel myself very much in sympathy with the general trend of his thought.

It seems to me that we should distinguish very carefully between two different applications of 'right'. Thus a statement is right when it fits or is consistent with the facts. Similarly, it is not right that a borrowed book is a gift, or that a man is a mere animal or machine. But, though something may be right in this sense, it is still possible to ask why it is *right for me* to treat it as such in my act. And it is doubtful if what is *right for me* can ultimately be divorced from human interest in some form or other. The deliverances of conscience are not to be taken at their face value: at least, we should seek to analyse them in the hope of avoiding the *impasse* of a 'heap of unrelated duties'. Perhaps the problem of the conflict of duties cannot be satisfactorily solved at the present stage of man's moral development, for every single theory of morals so far advanced seems too simple to account for all the facts. A comprehensive survey of the moral situation seems to reveal an attempt, however halting, at articulating a structure in which each and all will find their true place and function. On such a hypothesis due allowance is made for the principle of justice and for the principle of most good, but we are still left with the possibility of a conflict of duties in the present, though our knowledge of the conditions will enable us to make a fairly good guess at our absolute duty. The theory also accounts for our interest in the performance of duty, for it implies that ultimately things are not indifferent to one another, and that the interests of each are bound up with the interests of all. Though he may have no active desire to perform a particular duty the moral man will be moved to such performance by the fundamental interest implied in the conviction that he is acting in accordance with the scheme of things. It is the idealist's venture of faith.

We shall look forward eagerly to the further development of Mr. Carritt's views, and hope that he will clear up some of the obscure points in the present statement of his position. Meanwhile I wish to thank him for the assistance he has already given us, and have no hesitation in recommending his *Morals and Politics* as a very suitable text-book for students.

D. DAVIES.

Berkeley and Malebranche: A Study in the Origins of Berkeley's Thought.

By A. A. LUCE, D.D. Oxford University Press, 1934. Pp. xii + 214. 10s.

PROF. LUCE has written a book of note; and I hope to say here why it is noteworthy, and by whom it should be noted.

In broad terms the work is scholarly to a degree, as becomes a study of sources. Nor is it a heavy study, for it is couched in admirable prose. Prof. Luce gives the impression of being a judge dispassionately sifting evidence rather than a logician scrutinising philosophical arguments. But rarely does the author permit himself an infusion of personal feeling.

The purpose of the book—to establish Berkeley's philosophy in its proper perspective—may be divided into an attempt (1) to show how strongly

Berkeley was influenced by Malebranche, and (2) to exhibit Berkeleism as a whole.

The key-note of Prof. Luce's argument is the *Commonplace Book*. From this Berkeley's close study of Malebranche and the kinship in their views are established in the literary manner by internal evidence. Berkeley's every divergence from Locke is shown to have a literary and a conceptual counterpart in Malebranche. References are fully given. Since the case for (1) is established up to the hilt, I must pass over the several excellent chapters on the topics of Method, Theory of Vision, Immaterialism, and Abstract Ideas, to those chapters which more directly deal with (2).

Not only is it important to trace a similarity to Malebranche's conception of seeing all things in God, but it is necessary to show how this conception formed the basis of Berkeley's metaphysic—the "denial of matter" and the "affirmation of ideas". To these ends Prof. Luce gives in the chapter on "Seeing All Things in God" a clever interpretation of a much disputed passage in the *Principles*, § 1. The word "objects" is interpreted by him to have a wider denotation than it had for Locke, and confirmation of this is to be found in Malebranche. Knowledge of objects, on this view, includes knowledge of spirits. (It is however questionable if Prof. Luce gives a sufficient account of "such as are perceived by attending to the passions and operations of the mind" on p. 74). Of these spirits the principal one was God. We might at this point go to the section on knowledge of God in the chapter on "The Principles of Knowledge." The main point of a long, somewhat unclear, section appears to be that Malebranche did not require for knowledge of God's existence ideas as intermediaries. Locke's view, on the other hand, was that knowledge of God's existence was inferential. Hence Berkeley must have followed Malebranche. Reverting now to the first of these chapters, we find a statement of Malebranche's, "God is the intelligible world, or the place of spirits" (p. 80); that Malebranche "substituted the God of ideas for the world of ideas" (p. 83); and that for him ideas were counterparts of their Essences *necessarily* in God (p. 79). The point of all this is to add weight to the view that the concept of God for Berkeley was not a *deus ex machina* brought in to jump a difficulty in epistemology, but rather that the concept of God was the centre of the whole theory. Now pp. 84-5 are very important, because there Prof. Luce purports to show how his view of Berkeley removes all difficulty from understanding that Berkeley can consistently say two people are seeing the same thing. I cannot see that this new interpretation removes the difficulty.

A contrast might be made in this way. According to one common view of Berkeley, common sense requires that two people can be said to see the same thing, but as Berkeley's epistemology does not provide for this (without perhaps excluding it) it is necessary to postulate the existence of God. On this view, the existence of God, because inferential, is *less certain* than the fact that two people can be said to see the same thing. According to Prof. Luce's view, common sense requires that two people can be said to see the same thing; and the existence of God is certain (a proposition which somehow or other ensures the former). On *this* view the existence of God, not being in any way inferential, is *as certain as* (but not more or less certain than) the fact that two people can be said to see the same thing. But is Prof. Luce right? His statement at the top of p. 85 on the matter is not satisfactory if it is more than a mere statement of views. Prof. Luce gives us no reason to justify Berkeley's postulating the existence of God. But on his grounds he is probably right in not doing so, for the existence of

God was a certain, non-inferentially known, fact. That God exists and is known to exist may best be regarded as a primitive proposition of the philosophy, and the principal property with which God is endowed is the property of owning ideas.

This conception fixes Berkeley's epistemology (using the word to exclude knowledge of God's existence) as an element in a wider metaphysic and not as a theory on its own account. It throws new light on Berkeley's use of the word "idea", and therefore must be regarded as valuable. It has, however, a further far-reaching consequence. It is a conception which requires us to think of philosophies as wholes or as personal expressions of their authors, rather than as contributions to certain philosophical problems. The emphasis is on a psychological treatment of a man's outlook, whose special expression is a philosophic one—we ask what view of the *man* will link up his apparently divergent utterances.

This brings me to the task of classifying and evaluating the work. Those who hold that the value of a philosophy lies *solely* in the contributions it makes to specific problems are hardly likely to be interested in Prof. Luce's book. Those who hold that an important philosophy is a clue to the workings of a great mind will regard this work as an unusual achievement. It is worth adding that the two views are not incompatible—thus we might agree with Prof. Luce's interpretation while at the same time regarding Berkeley's contribution to epistemology as solipsistic. The work is difficult to classify. It can hardly be called a book on philosophy, since it does not deal with philosophic problems with a view to solving them. It is the sort of work which makes good histories of philosophy possible.

There are appendices on the *Commonplace Book* and Berkeley's visits to Malebranche, and there is an index.

J. O. WISDOM.

Geschichte der Mittelalterlichen Chinesischen Philosophie. VON ALFRED FORKE. Hamburg: Friedrichsen, de Gruyter u. Co., 1934. Pp. xii + 410.

EUROPEAN philosophers have the misfortune to be cut off from oriental philosophy by a philological barrier. The philologist does his best to bridge the gap; but it does not necessarily follow that the philologist tells the philosopher what the philosopher wants to know. A similar situation arose when the historians of art began to turn their attention towards the East. Obliging orientalist supplied a spate of entertaining anecdotes about the eccentricities of famous painters and craftsmen, but failed as a rule to tell the art-historian anything that really concerned him. Then came the demand of Strzygowski that the study of art, whether European or Oriental, should remain in the hands of the *Fachmann*, of the art-specialist, who must have the confidence to apply his trained sensibilities to all works of art alike, even at the risk of shocking the academic world by his ignorance of biographical, orthographical or historical niceties. The *Kunsthistoriker* was told to trust his own eyes, and to a great extent he has learnt successfully to do so. Unfortunately it is useless to tell the philosopher to trust his eyes. He possesses no trained susceptibilities that can be applied to the pages of a Chinese book, and however much he may desire to escape from the ministrations of the linguistic specialist, he is powerless to do so. This book, like Prof. Forke's work on early Chinese thought, consists almost entirely of translated extracts. It is in fact an anthology of passages from writers of the second century B.C. down to the

tenth century A.D., and there is a deliberate avoidance of comment or interpretation. Thus some at any rate of the disadvantages arising from the fact that Oriental thought can only come to us through the offices of a linguistic specialist, are successfully avoided. The choice, however, remains that of a Chinese scholar rather than of a philosopher. Moreover, Prof. Forke makes a completely arbitrary distinction between 'philosophers' and other writers such as historians, economists, etc., confining his extracts to what he calls philosophical writers, despite the fact that, as he himself admits, the conception 'philosopher' has never existed in China. What he sets out to do is, presumably, to write a history of Chinese thought, and it is surely of no consequence whether the material is gathered from histories, economic discussions or even from the works of commentators on the Classics, provided that such material is of philosophical interest. The long period dealt with and the huge number of works which survive might seem to justify even a somewhat arbitrary principle of selection. But the truth is that Chinese philosophy after the third century B.C. consisted very largely in a continual re-assertion of the same principles, and though the quantity of matter is large, the amount of new and interesting material is very small, so that no additional sources capable of supplying anything of significance or interest can safely be ignored. Native thought was in fact dormant, particularly during the last five centuries with which the book deals. The great intellectual achievement of the period consisted in the assimilation of Indian philosophy, and unfortunately Prof. Forke's treatment of Buddhism is very weak and superficial. A serious lacuna at the end of the book is the failure to show the origins out of which the Neo-Confucian philosophy with which Prof. Forke's next volume will deal, first arose. It is commonly asserted that the main source of inspiration was the works of Ch'ên T'u'an (died 989), who falls well within the period of the present volume. To what extent do these works survive, and have they in reality the historical importance usually ascribed to them? These are questions to which we may hope Prof. Forke will return in his third volume.

ARTHUR WALEY.

The Platonic Legend. By WARNER FITE. Charles Scribner's Sons, New York and London, 1934. Pp. viii, 331. 10s. 6d.

THIS is a book which should be very helpful to undergraduates desirous of getting a rise out of their tutors. For it is a systematic attempt to 'debunk' Plato, that is the 'orthodox' tradition about him in the histories of philosophy, as represented in W. R. Inge, A. E. Taylor, F. J. E. Woodbridge, B. Bosanquet, Paul Shorey and Elmer More (p. 2), and is written in Prof. Fite's best and most incisive manner. He was provoked, he tells us in his Preface, some four or five years ago by a colleague who questioned a casual statement of his that the Platonic Kallipolis provided education only for the guardians, and not for the masses. So he set himself to show that Plato was by no means an enlightened liberal, and to explode a number of other superstitions about Plato, such as that he was a great moralist, a profound mathematician, a consummate artist and a true Christian. In the main he seems to me to make out his case, though he produces little that should excite surprise in anyone accustomed to critical reflection on the traditional history of philosophy, and though the debunking hits Plato's

commentators more severely than Plato himself. For example the following about Jowett and Shorey seems distinctly neat. "Jowett's introductions are full of shrewd common sense and not always wholly favourable to Plato; his translations seem inevitably to drift towards constructions that will recommend Plato to English readers. In this he differs from Prof. Shorey, who may tell you in a footnote that Plato does not mean what he obviously seems to mean, but whose rendering of the text is scrupulous" (p. 32). Prof. Fite also seems to me to be plainly right in holding that the condemnation of Socrates was essentially political, and that Plato was essentially an intellectualist, in the sense of "one whose conception of intelligence takes the direction of formal logic and scientific method" (p. 180); only he should have added a warning that, strictly speaking, neither of these had as yet come into being. I was glad to find that he thinks that "the thirteen letters attributed to Plato are not such as to enhance the dignity of the philosopher" (p. 294), and that the Seventh Letter in particular is gravely discreditable. On the other hand I cannot agree that "Plato the man was not quite a man; . . . his point of view and his attitude towards life were very largely what would to-day be called adolescent, not to say childish, and what has been traditionally called 'feminine'" (p. 290), and that although he does not refer to the best argument for his contention, *viz.*, Plato's astounding proposal to put the philosopher-king in charge of all the children below the age of ten, after expelling the rest of the population. Nor can I agree that modern eugenists have overlooked their debt to Plato (p. 57), as he can easily convince himself by reference to my *Eugenics and Politics*. And I was a little shocked by his holding that Plato's was "the art of a clever but prosaic mind" (p. 283), and that "he is a scientific rather than an artistic genius" (p. 270, *cf.* p. 230). I should have thought it obvious that Plato was a poet, nay a great poet, and that this comes out nowhere more clearly than in his mathematics and metaphysics, *e.g.*, when he tries to identify the Good with the One; as J. A. Stewart said, the soul's adventures upon the Plain of Truth and in the Supercelestial Space are the purest poetry, but will not bear thinking out. But that does not prevent the Ideal Theory from constituting one of the great landmarks in philosophic history, while the invention of the conception of Pure Spirit must also be credited to Plato. With these two notions to his credit Plato can survive a good deal more debunking than Prof. Fite has inflicted on him. His book is in general free from misprints; I have noticed only 'autochthynous' (p. 27), 'Davies-Vaughn' (pp. 239, 324) and 'the ring of the Gyges' (p. 329).

F. C. S. SCHILLER.

The Popular Background to Goethe's Hellenism. By HUMPHREY TREVELYAN.
London: Longmans, Green & Co., 1934. Pp. xii + 108. 7s. 6d.

THIS book gives us the nature and genesis of the "Hellenic" atmosphere in which Goethe was reared, the background to the direct influence on his Hellenism of the ideas of Lessing, Oeser, Winckelmann and Herder, the ground of popular tradition on which he could take his stand while trying to form his own ideas on Hellenism. This ground, to judge from what Mr. Trevelyan shows us, was about as unsatisfactory, from the point of view of historical truth, as was that on which the Greeks themselves stood in trying to form their ideas about Olympus. Received enthusiastically in the sixteenth century in Germany as in the rest of Europe,

Greek learning was killed in the seventeenth century and at the beginning of the eighteenth, almost in the schools and universities and certainly as regards its general influence on life and letters, by linguistic pedantry, Pietism, Cartesian Rationalism and Utilitarianism. Its revival, with methods of teaching improved, by its very foes and with true humanistic ideals, started from about 1730 chiefly under J. M. Gesner and Christian Gottlob Heyne. Goethe's youth came too early for him to profit from the scholarship of this revival; he imbibed only its enthusiasm and for the rest had to breathe the popular atmosphere. The latter, in Goethe's boyhood, emanated chiefly from text-books on mythology, manuals of pre-revival days filled with the ignorant hatred and contempt of those days for all that was Greek; one such was written by Goethe's own uncle, Loën. This anti-Hellenism was worked up in France under Cartesian influence and propagated by Perrault, Fontenelle, La Motte, and above all Bayle in his Dictionary: the Greeks sinned against the manners and morals and the literary taste of Versailles, and Homer's description of Achilles' shield went counter to the rational laws of geometry. But from France also, as well as from Switzerland, came the new phil-Hellenism: from Boileau, Mme Dacier, Breilinger, Racine, Fénelon, Rousseau. It culminated in the setting up of the Greeks as the "hommes naturels" of the primeval Golden Age. In Germany, well reflected by Wieland, for the general public the old and new mingled curiously in Rococo-Hellenism, in which the idealisation of the Greeks was partly in the image of the men of the Golden Age but largely after the pattern of the German Anakreontiker, whose world was "composed of the vilest passions, yet so refined away that no passion, but only the vulgarity, remained"—at the worst anticipating Hollywood's ideal of "beauty", sex-appeal and "refinement", at the best elevating refined softness over robust strength. Such was the matter of the new enthusiasm for Hellenism which the enthusiastic young Goethe received. To it must be added the ideas of the Winckelmann of the *Gedanken über die Nachahmung der Griechen*—in short, the *schöne Seele* ideal; the *Geschichte der Kunst*, when Goethe came to know it, had not yet had time to influence the general atmosphere: before Winckelmann's labours the knowledge of Greek art was small and collections and publications very defective.

The reflection that is suggested by the reading of this very scholarly little book is that for the general public of Goethe's day (including Goethe himself and other imaginative writers) the concept "Hellenic" was as little historical as is that of "Aryan" for the modern Nazis; like the Nazi notion it signified no real interest in the past but was merely a weapon used in a fight to revolutionise the present; but like it, it entailed a terrific parade of pseudo-historicity. Does such a parade manifest a desire on the part of human beings in general, and of Germans in particular, to impose their will upon and to fashion not only the present and future but—a thing which, the Greeks tell us, even a god cannot do—also the past?

P. LEON.

Immanuel Kant's Critique of Pure Reason. Translated by NORMAN KEMP SMITH. Abridged Edition. London: Macmillan, 1934. Pp. vii + 339. 10s. 6d. net.

THE translation of which this work is an abridgement was reviewed in an earlier number of MIND.¹ It is consequently not the task of the present

¹ xxxix. 384.

reviewer to say anything about the merits of the translation, but simply to indicate whether he thinks the selection of passages to omit or include judicious. This is a task that requires little space, and readers are entreated not to assume that the size of the review is in proportion to my opinion of the merits of the work!

Even those who are generally opposed on principle to the abridgement of great philosophical works should not grudge the lightening of the student's labours which Prof. Kemp Smith provides. For Kant is one of the philosophers who suffers least from an abridgement, partly because he often said what he has to say at excessive length, partly because certain portions of his work, especially his "architectonic", are clearly of no more than historical interest. And the passages given certainly include everything that is necessary to the ordinary honours student, at least unless he is taking the Critique as a special subject. The book is eminently suited for its purpose, and will play a very large part in our university studies and be the chief means of familiarising with Kant those also outside the universities who have developed an interest in philosophy. No doubt no two people would be likely to make quite the same selection, and I could mention a number of minor points where I should rather have inserted something which is omitted or omitted something which is inserted. *E.g.*, I cannot understand why the first paragraph of the Postulates of Empirical Thought, including Kant's important statement that the addition of a modal category does not increase the content of a concept, or again the Observations to the Antinomies, were left out, and I should myself have been inclined to cut down the section on the Paralogisms more, and certain other sections slightly less, than Prof. Kemp Smith does. No indication where omissions occur is given, beyond what is provided for an attentive reader by the numbers of the first and second edition pages printed by the side of the text. Some would no doubt demand a clearer indication by means of, *e.g.*, dotted lines, but I suppose it was thought that this would destroy the appearance of continuity which, despite frequent omissions of a few sentences here and there, attaches to the exposition. The book—and this is a considerable merit—does not read like an abridgement. It is perhaps worth while informing readers of this review that the proportion of the whole Critique included is rather less than two-thirds, omissions being naturally most frequent in *The Transcendental Doctrine of Method*. A large proportion of both the first and second edition *Transcendental Deductions*, and of both the first and second edition versions of the Paralogisms, has been included.

A. C. EWING.

Science et Loi, Cinquième Semaine Internationale de Synthèse. Paris, 1934. Felix Alcan. Pp. vi, 228. 20 fr.

THIS book appears to be the fruit of a course of lectures and discussions organised in the Hotel de Nevers in Paris by the 'Fondation "Pour la Science"', in which a number of well-known French scientists took part. The general idea was to have a lecture on the conception of law in a representative science, followed by a short discussion by experts, and the programme was carried through in the week from 29th May to 3rd June, 1933. The topics were the history of the notion of law (Abel Rey), law in the mathematical sciences (F. Gonseth of Zürich), law in mechanics and astronomy (H. Mineur), law in physics and chemistry (A. Berthoud

of Neuchâtel), law in biology (L. Cuénot), law in psychology (H. Piéron and H. Wallon), law in sociology (M. Halbwachs), and chance in history (V. Chapot). In addition to these authorities, MM. H. Berr, P. Langevin, F. Simiand and L. Febvre repeatedly intervened in the discussions. The whole gives a good idea of the problems which are at present engaging the attention of French scientists, and will no doubt have a clarifying effect on scientific methodology. Still it is a matter for regret that all these eminent professors should have confined themselves so conscientiously to their special topics that they had nothing to say about the more general and philosophic aspects of the problem of law. In consequence no very obvious 'synthesis' of the various notions resulted. It is significant also that there was no lecture on the *logical* notion of law, and that though the lecturers all professed complete empiricism none of them even mentioned the 'operational' function of 'law', as essentially a convenient instrument of calculation and control. The nearest they got to considering a general problem of scientific method was when they proclaimed (as they nearly all did) their fidelity to determinism, Heisenberg notwithstanding; but even this was not defended in any detail.

F. C. S. SCHILLER.

Phänomenologie und Metaphysik: Das Problem des Relativismus und seiner Überwindung. Von Arnold Metzger. Halle a. S. Max Niemeyer Verlag, 1933. Pp. xvi + 269. M. 9.

It is difficult to say precisely what phenomenology is, for philosophers who differ very widely among themselves nevertheless call themselves phenomenologists. Some phenomenologists appear to be realists, some embrace a sort of monadology, and some hold that we cannot attribute apodeictic being even to transcendental selves. The casual observer would say that there has been a transformation of the nature of phenomenology from Husserl's *Logische Untersuchungen* to Heidegger's *Sein und Zeit*. One aim of Dr. Metzger's book is to discover what phenomenology really is, and to explain its importance in the history of philosophy. As the book proceeds it becomes necessary to show the importance in modern philosophy of relativism and positivism, and to indicate the grounds of the opposition of phenomenology to these philosophical tendencies. In the course of the discussion the author points out that phenomenology, although it is opposed to positivism, nevertheless owes something to it. In particular, both positivists and phenomenologists aim at an approach to facts that is free from assumptions and presumptions. But positivists are prejudiced in favour of sense objects and what is immediate. Dr. Metzger is of the opinion that the true development of phenomenology is towards some type of idealism.

Whether this book is an independent philosophical work or an essay in the history of philosophy it is hard to say. A great deal of space is occupied with comparisons of phenomenology and other types of philosophy, and with attempts to 'place' it in the history of systems. While it is quite natural to compare phenomenology with the philosophy of Descartes, it seems rather laboured to spend time in showing the importance of Nicholas of Cusa in the history of modern thought. Thus the argument proceeds in an extremely leisurely manner and with much repetition. Those who have not some previous acquaintance with phenomenological literature are not advised to read this book except, perhaps, for the ap-

pendix on the philosophy of Max Scheler. For it is a book which presupposes in the reader a considerable knowledge of phenomenological theories.

H. B. ACTON.

La Pensée : Vol. II. (Les Responsabilités de la Pensée et la Possibilité de son Achèvement.) By M. BLONDEL. Paris: Alcan, 1934. Pp. 558. 60 fr.

If the text (as opposed to the *excursus*) in the first volume of this work (as I thought when I reviewed it for *MIND*) was rather too condensed, the text of this second volume is undeniably diffuse; and I do not think the second volume is by any means as good as the first. It abounds in warnings against misconceiving the subject, against blind realism, visionary mysticism and intoxication with the *élan* of life; but the reader who has grasped what he should *not* do, receives far too little instruction (I submit) concerning what he should do; although I allow he is told repeatedly that the gravity of the issues is beyond all dispute. Again, there are repeated warnings against the dangers of metaphor and of allegory, and repeated apologies for the use of these; but what we get, other than allegory, is muted and very pale. Indeed the behaviour of the currents and the colours in the Lake of Geneva is exploited to a tiresome extent, and could never be more than an illustration of the "diplopia" and "dichotomy" of concrete-impulsive versus abstract-notional *pensée* (regarded cosmically rather than psychologically).

The *excursus* too—about a hundred pages of them—seem to me to be much less valuable than in the first volume (where their value was very considerable). For the most part, they are replies to criticism of the first volume, always majestic but seldom pointed.

Perhaps, however, I have sinned by looking for what shouldn't be there. I hasten, therefore, to add, that philosophers who cannot breathe easily except in a *very* rarefied atmosphere may well have an opinion quite different from mine regarding the merits of this book.

A work on "Being" is to follow, and may clarify the theme. These two volumes concerning *la pensée* are obviously incomplete, and seem to be designed to be precursors of the work on *l'être*.

JOHN LAIRD.

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VII.—NOTE.

NOTE ON THE RELATIONSHIP BETWEEN LOCKE AND DESCARTES.

1. My colleague Prof. Koebner, formerly of the University of Breslau, while working on the Locke manuscripts in the Lovelace Collection, came upon a good deal of new material affecting John Locke. These papers, as is well known, have been looked through at various times (it was among them that both Lord King and Fox Bourne, and in our own day Benjamin Rand, found the documents they published), but it would appear that they would still repay the interest of any one who could devote to them the necessary time and attention. I am writing this note on one of the points raised by Prof. Koebner in the hope that it may stimulate someone to go into the whole question more thoroughly, taking all the available sources, including the Shaftesbury papers, into account.

2. The point in question is this. Some of these papers, particularly the large quarto (Lovelace MS. Q. IX., 7) which contains some other material already published, suggest that Locke made a special study of Descartes during his travels in France (1675-79). The problem is whether the question can be solved by reference to our other sources, and whether it is of any importance. I think that it can be solved and that it is of importance.

3. The *old view* of the relationship between Locke and Descartes is that Locke represents the 'empirical' reaction against Cartesian 'rationalism', and this was supposed to be shown in the Essay, particularly and typically in Book I, the attack on 'innate ideas'. It is clear, however, that Locke is also a 'rationalist' (cf. the clear-cut distinction between 'knowledge' and 'opinion' which is the basis of his whole theory of knowledge), and the 'innate ideas' he attacks have been shown to be not (primarily) those of Descartes (on this see already Hume's *Enquiry concerning the Understanding*, section II., last note). It is essential, therefore, before considering the question of the relationship between the two thinkers, to agree on points of similarity and difference between them which could serve as criteria for discussion.

4. *Points of similarity*: The most characteristic would seem to be (a) the conception of 'demonstration' as the last link of a chain of 'intuitions', and (b) the use made of the immediate apprehension of the existence of the 'self'. The former, the mainspring of the Cartesian logic, is found in its most explicit form in the *Regulæ*, the latter, the starting point of the Cartesian metaphysic, in the *Meditations*; but they are both clear enough in the *Discourse on Method* (the point is of some importance in view of the date of the publication of the *Regulæ*, although it seems clear on other grounds that the *Regulæ*, although not printed until 1701, circulated in manuscript before that date, and may well have been seen by Locke).

5. *Points of difference*: (c) Locke's refusal to accept Thought as the essence of mind; (d) his parallel refusal to accept Extension as the essence of body; (e) his severance of physics from mathematics.

6. Leaving aside for the moment the Draft of the Essay published by Rand and dated 1671, and taking the extracts from Locke's papers published by the biographers, it would seem clear *at least* that Locke was interested in, and made *some* study of, Descartes, during the years he spent in France. For example: (a) the *Journal* for 22nd March, 1676 notes 'the new philosophy of Des Cartes prohibited to be taught in universities, schools and academies' (King (ed. 1830), i., 119); (b) the *Journal* for 27th March of the same year shows him occupied with the problem of space and body, and we have the phrase 'imaginary space' which reminds one of the 'imaginary spaces' of Discourse V. (*ibid.*, 123). Much more important is (c) the essay on Knowledge (8th Feb., 1677). This begins with the 'QUESTION: How far, and by what means, the will works upon the understanding and assent'. On this it may be observed that (i) it is purely Cartesian; (ii) nothing like it is to be found in the Draft; and (iii) the addition of the last two words to the normal Cartesian question indicates the special interest which is Locke's own. The passage is further interesting in that it contains Locke's judgement on physics ('we cannot understand the essence of things') which is to be found both in the Draft and the Essay.

7. We may now turn to the Draft itself (remembering its date, 1671) and examine it with reference to our specific points.

(a) *The conception of demonstration*: This is *not* in the Draft, where there is no idea of a 'chain'. On the contrary, 'demonstration' is 'intuition' (p. 103), *i.e.*, the actual seeing ('showing men how they shall see right', 104), and so on pp. 97 f., 101 f., 105, 108, 111 f., 117 (contrast with the stress on mediation in Essay IV., 1, § 9; 2, § 2 f.; 3, § 18, etc.).

(b) *The intuition of the 'self'*: 'This being, according to Descartes, to everyone past all possibility of doubt, that while he thinks or thinks that he writes, he that thinks does exist' (p. 88). But it should be noted that this reference is quite incidental and is *not*, as it is in the Essay, the explicit ground of the proof of the existence of God (treated of in pp. 206-07), although the ontological argument is rejected in the Draft (p. 207) in the same way as it is in the Essay.

(c) *Locke's refusal to accept thought as the essence of mind*, and (d) *the parallel refusal to accept Extension as the essence of body*: in the Draft (pp. 64-65, 198-206) we have the same so-called materialistic doctrine as in the Essay, but nothing of its specific anti-Cartesian polemic.

(e) *The severance of physics and mathematics*: in the Draft we have the same general doctrine of substance ('collection of simple ideas'; 'unknown cause', etc.; cf. pp. 64, 123, 127, 165, 195, 198, 202) which we find in the Essay, together with the corollary that there are 'no universal propositions concerning causes and effects' (p. 279 f.). But we have nothing of the Essay's main point that mathematics is certain because it is abstract, physics uncertain because it is particular, and nothing of the Essay's well known attempt to present a view of morals as a deductive science on the model of mathematics (for the Draft's view of morals, cf. 291 ff.). On the contrary, the Draft insists that knowledge is of real existence, *i.e.*, 'apprehending things as they really are or do exist' (pp. 85, 300).

8. It would seem to follow that Locke's special study of Descartes began after the completion of the Draft in 1671, and was strong at the beginning of 1677. Pending a more systematic examination of the Lovelace and Shaftesbury papers and a more detailed investigation of the Draft in connection with the Essay, the general opinion may be expressed that while there is no doubt that when Locke wrote the Draft he knew of

Descartes (and who could not in 1670?), he had not made any special study of him or ever realised how important for his own enquiries Descartes' results were. Indeed, the phrasing of the only specific reference to Descartes in the Draft (quoted above), as, too, of the oblique references (132, 207), is compatible with the most cursory acquaintance, even by hearsay. But having written the Draft and having the leisure of travel, particularly in France, Locke turned specifically to Descartes and sharpened his own views on Descartes' writings, reaching only then the characteristic doctrines indicated above (§ 4) while retaining his native independence (§ 5). It may be noted in this connexion that the *Thoughts on Education* (1690) still consider Descartes primarily as a physicist (§ 193).

9. It seems to me that here we have the real explanation of the old puzzle why Locke delayed so long in publishing the results of his meditations. It is generally assumed that his views remained unchanged from the time of his famous talk with his friends on the subject of 'morality and revealed religion' till the date of the publication of the completed Essay. The publication of the text of the Draft was thought to confirm this view, and indeed it may well be that Locke's views on fundamentals did not change. But it is also clear that his views on many important subsidiary questions did change (for the fact see R. I. Aaron, *Proc. Arist. Soc.*, 1932-33, p. 196 ff.), and it may well be that he delayed publication until he had cleared them up.

10. It should be added that while the points indicated in §§ 4-5 are simple and striking, and are offered as ready criteria for this special problem, it remains that the aims of the two thinkers are essentially different. In spite of *Regulæ viii* Descartes was not interested in the 'critical' problem; and Locke entered into the 'critical' problem from a primary interest in ethics and religion. Locke was therefore in any case only likely to use Cartesian ideas in order to clear up his own, and that is the view that would seem to emerge from the considerations offered above.

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Jerusalem, May, 1935.